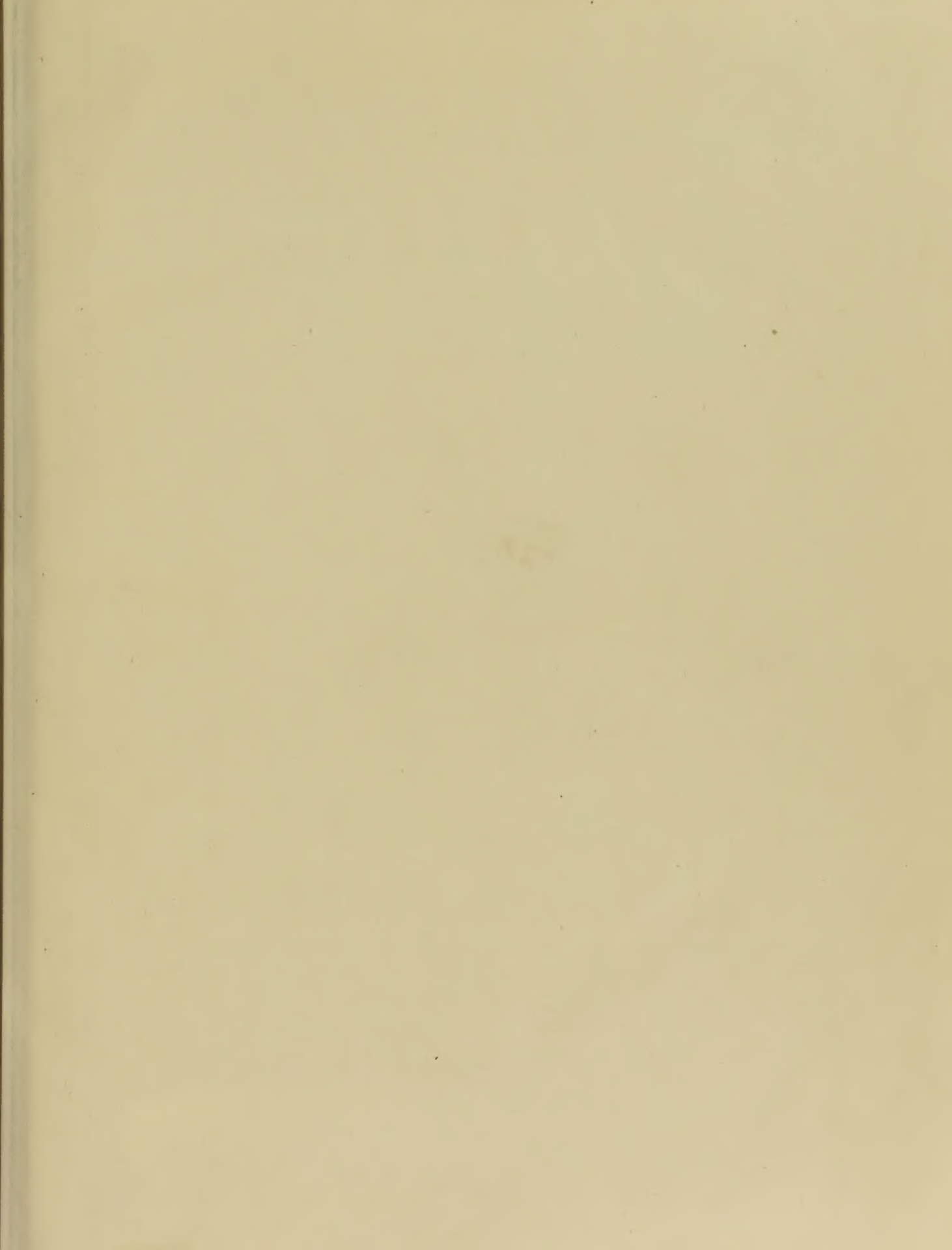




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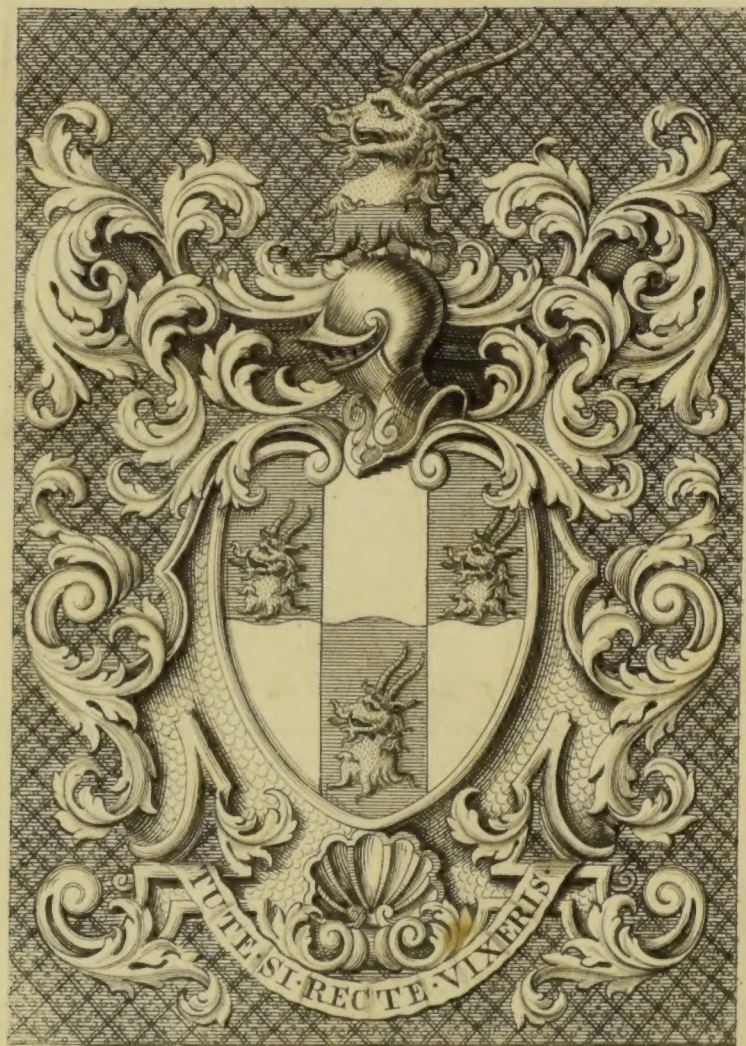


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PHILOSOPHICAL
TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Confiderable Parts of the WORLD.

VOL. LI. PART II. For the Year 1760.

L O N D O N:

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M.DCC.LXI.

PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF LONDON

AND OF THE

ACADEMY OF SCIENCES



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OF THE

C O N T E N T S

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P A R T II.
For the Y E A R 1760.

XL. *A Letter to the Honourable J. Th. Klein, Secretary to the City of Dantzick, from Mr. Peter Collinson, F. R. S. concerning the Migration of Swallows.*

Read March 9, 1758. **I** Do not know which to admire most in my dear friend Klein, his learning, or his judgment, in compiling so many ingenious and instructive books in natural history.

But I must beg leave to dissent from my learned friend, in an article he takes great pains to establish; which is, that swallows are not birds of passage; but, at the time of their going away, retire under water, and live therein, all the winter. This I cannot comprehend, being so contrary to nature and reason; for as they cannot live in that state, without some degree of breathing, this requires the circulation of the blood, however weak and languid. Now, as respiration is absolutely necessary for circulation, how is it possible to be carried on for so many months under water, without the risque of suffocation?

Besides, if so remarkable a change was intended, the great wisdom of the Almighty Creator would, undoubtedly, be seen in some particular contrivance, in the structure of the organs of the heart of this bird, to enable it to undergo so very remarkable a change of elements: and my learned friend has not attempted to show any thing of this nature, in order to confirm his system.

An easy experiment may throw some light on this doubtful affair—At the time of their going away, take a swallow, and confine it in a tub under water: if it remains there for a week or two alive, without any remarkable inconvenience, then there may be some probability for its continuing so many months in that state.

The conclusions, that are drawn from some of the tribe of insects subsisting under water, are far from being conclusive, to found an analogy upon; as insects differ from other animals in so many particulars, that very little or nothing can be concluded, or inferred, of the one, from what we observe in the other.

Towards the end of September, the swallows assemble on the reeds in the islands in our river Thames, and have, no doubt, so done for ages past; and yet I never heard, or read, of any fisherman, or other person, that has ever found in the winter months a swallow under water, in a torpid living state; for if such a marvelous thing had ever happened, it would have been soon communicated to the public. Besides, as these islands of reeds and willows are annually cut down, for several uses, and yet not a swallow has been discovered in his aquatic abode; and, considering

ing the multitudes I have seen on these reeds and willows in the autumn; if they took their winter's residence under water, it is most reasonable to think, in a river so frequented, and in so long a course of years, some would have been found in that situation. Another circumstance I must add; in great towns remote from water, where rivers and reeds are not near, I have frequently observed that, a little before the swallows depart, they, every morning early, gather together on the roofs of large houses, exposed to the morning sun: this they daily do for some time, to collect themselves, before they take their flight.

Next, to confirm my opinion, that the migration of some species of swallows is certain, I think I have some undoubted proofs.

I have often heard Sir Charles Wager, first lord of the admiralty, relate that, in one of his voyages home, in the spring of the year, as he came into soundings in our channel, a great flock of swallows came and settled on all his rigging: every rope was covered, they hung on one another like a swarm of bees; the decks and carvings were filled with them; they seemed almost spent and famished, and were only feathers and bones; but, being recruited with a night's rest, they took their flight in the morning.

Capt. Wright, a very honest man, whom I could depend on, told me, the like happened to him, in a voyage from Philadelphia hither.

But a yet stronger confirmation of the swallows being birds of passage is the observation in Mr. Adanson's history of Senegal, lately published; which is, as near as may be literally translated, from the author's own words; *viz.* " The sixth of the same month,

“ (October) at half an hour past six in the evening,
 “ being about fifty leagues from the coast, (between
 “ the island of Gorea and Senegal) four swallows
 “ came to take up their night’s lodging on the ship,
 “ and alighted on the shrouds. I easily caught all
 “ four, and knew them to be the true European
 “ swallows. This lucky accident confirmed me in
 “ the opinion I had formed, that these birds pass the
 “ seas, to get into the countries of the torrid zone, at
 “ the approach of winter in Europe; and, to that
 “ purpose, I have since remarked, that they do not
 “ appear at Senegal but in that season. A circum-
 “ stance no less worthy of note is, that at Senegal
 “ the swallows do not build nests, as in Europe; but
 “ lie every night by pairs, or single, in the sand upon
 “ the sea-shore, where they rather chuse to fix their
 “ habitation, than up in the country.” *Hist. de*
Senegal, p. 67.

This observation (as it comes from a professed naturalist, and one, who went into those countries on purpose to collect what was curious in that way) seems to put the matter out of doubt; and the hear-say stories of ignorant peasants and credulous people are by no means to be put in competition with it.

I have, for many years, been very watchful in taking notice of the times when the swallows leave us, and have twice seen them undoubtedly taking their flight. At two different years, on the 27th and 29th of September, walking in my garden at noon, on very clear sunshiny days, and looking up into the sky, at a very great height I distinctly saw an innumerable number of swallows, soaring round and round, higher and higher, until my eyes were so
 pained.

pained with looking, that I could no longer discern them.

But, as my friend Klein seems to be so positive, that the *hirundo riparia*, or sand martin, at the approach of winter, retires into the holes, in which that species breed up their young, and made their summer's residence, and there pass that cold season in a dormant state, as snakes, lizards, and some other animals do, I have been the more solicitous to come at the truth. But as these sandy precipices, in which these martins build, are mostly inaccessible, some years have passed, before I could find a situation where the experiment could be fairly made, without difficulty or danger. Such a sand-hill I found in the parish of Byfleet in Surry. The clergyman being my friend, and well qualified to make the experiment, at my request, was so obliging to undertake it. I shall give his letter to me, in his own words.

“ Dear Sir,

“ Byfleet, October 22, 1757.

“ I took a square of about twelve feet, over that
 “ part of the cliff where the holes were thickest,
 “ which, in going down from the surface, I judged
 “ would take in about forty holes. I set to work,
 “ and came to the holes; but found no martins,
 “ nothing but old nests in the furthest end of the
 “ holes, which were from a foot and half to two
 “ feet and half deep from the entrance. We care-
 “ fully searched forty holes, but found no birds;
 “ but at least thirty of them had nests. The passage
 “ to them was very near in a straight horizontal line;
 “ the nest was sunk about an inch and half below
 “ the level of the passage; the materials next the
 “ bottom.

“ bottom were straws, then coarse and fine grasses ;
 “ the whole structure of no great elegance. The few
 “ eggs, that were left behind, were of a clear un-
 “ spotted white, the size of a robbin-red-breast's.”

This fair trial being made by a gentleman of veracity and ability is very conclusive ; for it certainly proves, that the sand martins do not take up their winter abode in their summer dwellings. Therefore, there is sufficient reason to believe, from the before-recited observations *on the common swallows*, and *this* so recently made on the *sand martins*, that they are all birds of passage.

ADDITIONAL REMARK.

There are four distinct species of birds, that go under the general name swallow ; viz. the swift or black martin ; 2. the swallow, that builds in chimneys ; 3. the martin, that builds against houses ; 4. the sand martin, that builds in sand-banks. I hope, that I have clearly proved, that some of these species are birds of passage. But some of my friends assert, that they pass the winter in clefts or caverns of the earth, in banks or precipices. What is much to be regretted is, that the gentlemen were not curious enough to distinguish the particular species, which they found in a torpid state. Mons. Adanson, in his account of Senegal, has omitted this. So that nothing certain can yet be pronounced, which species stays, or which goes.

XLI. *Observations on the Comet seen in January 1760; by James Short, M. A. F. R. S. In a Letter to the Rev. Tho. Birch, D. D. Secretary to the Royal Society.*

Dear Sir,

Surry-Street, 10 Jan. 1760.

Read Jan. 10,
1760.

A Comet has made its appearance near the constellation of Eridanus, a little to the westward of Orion. Last night I took its transit over the meridian, and likewise its declination. Its nucleus is small, subtending an angle of not more than 5 or 6 seconds, but very visible thro' a two feet reflector magnifying about 70 times. Its motion is to the westward, with a considerable velocity, seemingly about 2 degrees in a day; for, about an hour and a half after I had taken its transit, I judged it had advanced about 10 or 12 minutes; which I find to be about the rate of the great comet, when it first was seen in the end of the year 1743. This comet is very visible to the naked eye, tho' I could perceive nothing of a tail; and therefore I conclude it is going down to the sun.

Comet passed the meridian 9th January	} Mean time.
1760, at - - - - -	
Its declination south - - - - -	9 ^h 8' 53"
Rigel Orionis passed the meridian at -	2° 15' 0"
	9 ^h 47' 49"

I am, Dear Sir,

Your most humble servant,

Ja. Short.

XLII. *Observations on the same Comet; by the Rev. John Michell, M. A. Fellow of Queen's College in Cambridge. In a Letter to Mr. James Short, F. R. S.*

Dear Sir,

Read Jan. 10, 1760. **T**Hough in all probability you will have seen the comet, which is now apparent; yet, lest you should not, I was not willing to omit this opportunity of informing you of it. I received a message last night from Dr. Mason, who, as far as I find, was the first here, that discovered it. We did not suspect at first, that its apparent motion was so great, as it appeared to be on examination; or else should have taken more observations. Very luckily, however, I took its distance from four principal stars, between a quarter and half an hour after nine, which was as soon as I saw it; and, about an hour and a quarter after, finding that it had manifestly moved, to the naked eye, I took other observations of it, every quarter of an hour, or twenty minutes, till very near two o'clock, when it ceased to be visible, on account of its being too low in the mud.

The first observations gave its distance from κ Orionis, $3^{\circ} 29'$; from Rigel, $11^{\circ} 46'$; from Betelgeuse, $17^{\circ} 10'$; and from Sirius, $12^{\circ} 56'$. All these observations were made between a quarter and half an hour past nine, and, as well as I could recollect, in the order I have set them down; which, not imagining

gining it was material, I had not been careful to mark to single minutes. At $1^h 22'$, its distance from Rigel was $7^\circ 6'$; at $1^h 24'$, from Betelgeuse $15^\circ 53'$; and at $1^h 36'$, its distance from Sirius was $17^\circ 36'$.

This will be sufficient for you to find its place by. The observations, together with above twenty others, were made with a little Hadley's quadrant, and may most of them, I believe, be depended on to about two or three minutes; but some perhaps may err four or five minutes.

I am, Sir,

Your obedient servant,

Cambridge, Jan. 9, 1760.

John Michell.

XLIII. *An Account of the same Comet; by*
Nicolas Munckley, of Lincoln's-Inn, Esq;

Read Jan. 17, 1760. **J**ANUARY 9, 1760, I observed what appeared to me to be evidently a comet, west of the constellation of Orion, or (to speak more astronomically and exactly) over the two stars marked μ and ν in the river Eridanus, but nearer the latter than the former; right ascension, about 66 deg. declination, about 3 deg. S. It was something dimmer and larger than either of these stars; and through a telescope, appeared magnified, and surrounded with a broad, faint, ill-defined haziness, like the last comet, such as plainly distinguished it from any

VOL. LI. P p p thing

thing else in the heavens. It seemed, even between the times I observed it that evening, to have a sensible motion towards the north-west (*i. e.* nearer the zenith, and contrary to the order of the signs): for though at first I apprehend its place to be as above, on reviewing it, two or three hours afterwards, it was more over the star ν , and a little to the westward of it.

This phænomenon was seen the night before (*viz.* the 8th, when I was in London), by two or three persons, who mentioned it to me: but though they particularly took notice of it as different from other stars, they were not astronomers enough to give any exact account, either of its place or appearance.

The evenings following the 9th were cloudy, till the 13th, which, though far from being very clear, allowed me however, at times, to see the constellation Orion, and westward of it, as far as Cetus, &c. I saw particularly the stars μ and ν , mentioned before, and the places above-referred to over them; but I saw nothing any more of the comet.

Hampstead.

Nicolas Munckley.

XLIV. *An Account of the said Comet; by*
Mr. Mark Day.

S I R, Lowick, near Thrapston, Northamptonshire.

Read Jan. 17, 1760. **I** Had the pleasure last night, about five o'clock, to observe a comet in the southern hemisphere, near the northmost extremity of the river Eridanus, tending its course towards Pegasus, and, near as I can guess, it will cross the ecliptic about 20° in Aries. It moves one degree in less than one hour and half; but seems too hasty to give the astronomers leave to make many observations upon it, unless the weather prove favourable. If there has been any accurate observation thereupon, I would gladly know; but if it has not been taken notice of, then I hope the above will be acceptable from,

S I R,

Your very humble servant,

Jan. 10, 1760.

Mark Day.

It passed the meridian about 9.

XLV. *Of the vitriolic Waters of Amlwch, in the Isle of Anglesey; with occasional Remarks on the Hartfell Spaw, described in the first Volume of the Edinburgh Essays and Observations physical and literary, and in the 49th Volume of the Philosophical Transactions, and their Comparison with other Waters of the same Class.* By John Rutty, M. D.

Read Jan. 17, 1760. **T**HE truly vitriolic waters, altho' by much the strongest of the chalybeates, are so rare in comparison of the ordinary chalybeates, that we have scarce any accounts of them in the memoirs of the learned, nor any particular histories of such waters, until of late; and therefore, it is presumed, that an additional account of a certain notable water of this class may not be deemed unworthy of being preserved.

Such a water is that of Amlwch, situate on Traflwyn mountain, in the parish of Amlwch, in the Isle of Anglesey, which was sent me by Ambrose Lewis, of Beaumorris, having been bottled May 31, 1757, and arrived in Dublin June the 3d following.

It appears, by the hydrometer, to be as light as distilled water, notwithstanding its strong impregnation.

It is of a subacid taste, and very nauseously vitriolic; a lasting impression of that sort continuing in the throat giving suspicion of copper, of which, however,

however, it exhibits no evidence, by any degree of the hue of that metal imparted to polished knives immersed in it, nor of sulphur, by discolouring silver. It retained the above-mentioned taste, on being exposed several days in an open vessel; quite otherwise than happens to our ordinary chalybeates.

It curdled with soap: it also curdled equal parts of milk, exhibiting a pretty clear whey.

With spirit of hartshorn, spirit of sal ammoniac, and the solution of potashes, it exhibited ochreous and green grumes, as the martial vitriol.

Its appearances with galls, and other austers, were very singular, and worthy remark: for tormentil-roots gave it only a dilute ink colour, soon fading; and green tea a dark dun colour, on standing. It had not blackened the corks, except perhaps one out of six, that stopped the bottles sent me; and when the water was first poured out, it struck no more than a slightly bluish tincture with galls; which tincture, on standing all night, became like a dilute ink; but in a glass exposed thirty-two hours, the galls struck the dilute ink colour sooner: and in some of the water, which I left exposed four days, and in some of it, which I left in a phial corked, but only three quarters full, three weeks, the effect was very different; for to each portion of water so exposed, as I have just now mentioned, the galls imparted a most beautiful bright sky blue; which blue tincture, a little spirit of vitriol instantly destroyed.

Hence we may see the fallacy of trusting to a few appearances, and the danger of rash conclusions, without repetitions of experiments and observations; which, if they had been omitted, I had been led to conclude

conclude this water to be but slightly impregnated with either a chalybeate or vitriolic principle; with which last it is, however, assuredly strongly saturated, altho' the ordinary taste with galls does not discover it clearly by the blue tincture, until the dissolving acid is partly exhaled.

The ANALYSIS.

It deposits an ochre, which serves for painting.

It also deposits in the bottles a sediment* partly of a red and yellowish colour, and partly white and raggy; which sparkled, and smelt strong, on the red hot iron.

Two pounds eleven ounces yielded 16 grains (*i. e.* a gallon 49 grains) of a light green sediment, of an acid smell, and of an highly acid, vitriolic, and nauseous taste.

It ferments strongly, both with solution of potashes, and with spirit of sal ammoniac; and separates a green and ochreous matter with the last.

Galls added to its dilutum in distilled water turned it of a deep blue; the characteristic of martial vitriol, to which it also agrees in the experiments of the last paragraph.

It produced some degree of coagulation with albumen ovi; and some slight opacity, and small grumes, with saliva.

It turned of a brown greenish colour with syrup of violets.

It was not attracted by the magnet, until roasted in the crucible; and then it was strongly attracted, and turned as red as minium.

From

From the above experiments, it appears, that Amlwch water is strongly impregnated with an acid martial vitriol.

It kills all the fish in its passage.

It has sometimes been drunk; but cannot be borne in a greater dose than half a pint, unless diluted with common water, being otherwise vomited up.

It cures the mange in horses, and the itch in men, by bathing.

From the above account of the Amlwch water, it appears evidently to agree to that of the Hartfell, above-mentioned, and described in the Edinburgh Essays, and in the Philosophical Transactions, even in certain distinguishing characters common to both, and in which they differ from our ordinary chalybeate waters; *viz.*

1. In the acid and vitriolic taste, which, moreover, they retain, when long kept, and at a distance from their fountains; and even upon boiling, yielding an acid vitriolic salt, on exhaling to dryness, which the common chalybeates never do, but lose their strength by a small degree of heat.

2. In the blue tincture, which they give with galls; another distinguishing character of English vitriol, of which a weaker solution, like the common chalybeates, gives only the purple colour with galls.

3. In exhibiting green clouds, or grumes, with oil of tartar, like the martial vitriol.

The same, or like appearances, are exhibited by the Shadwell water; by another at Swanzey in Wales; and, upon a late diligent search into the waters of this kingdom, by those of Kilbrew, in the county of Meath; of Ballymurtogh, in the county of Wick.

low; and of Cross and Coshmore, in the county of Waterford, and some others: and, to conclude, as crystals of martial vitriol have been demonstrated in several of them, I shall not hesitate to pronounce them *acid vitriolic waters*; which waters, as they are new in practice, and different in operation and effects from the common chalybeates, I apprehend it will be worth while to endeavour to place them in a more conspicuous point of view, which the histories lately given of the British waters of this class above-mentioned, compared to our own, will enable us, in some measure, to do.

There are, indeed, in these waters different degrees of acrimony; for tho' most of them are so acid as to curdle milk, yet this is not altogether universal; and tho' in many of them the acid is so far developed in their solid contents, as to ferment with alkali's, this appearance is not always conspicuous; from whence one would imagine, that the milder sort might be used with more freedom, or less danger: and yet it is certain, that one of the sharpest of them all, *viz.* ours at Kilbrew, in the county of Meath, hath been taken inwardly, with amazing success, in some very stubborn cases.

Yet, on the other hand, it hath been observed, that even the German spaw sometimes hath proved too irritating in some tender constitutions, where our ordinary milder chalybeates have succeeded well; and we are otherwise informed, by an accurate observer, that, in some tabid cases, particularly that called the galloping consumption, the mildest and lightest of our own chalybeates, and even altho' blended with milk,
have

have been found to increase the hectic heats and tension of the pulse.

Now, this observation seems not easily reconcilable to another of Dr. Horsburgh, in the place above-mentioned, on the Hartfell spaw (a much stronger and harsher chalybeate than either the German spaw, or any of our ordinary chalybeates), *viz.* that it hath actually been given, with notable success, from half a pint to a pint a day, in consumptions of the lungs, far advanced, even attended with hectic heats and night sweats.

So memorable a fact, in the cure of a deplorable disease, deserves attention; and the Scotch physicians in that neighbourhood are called upon to corroborate it by further observations; as how long those cures stood, and how far they may have been confirmed by the like success in similar cases; whether used with or without milk; and lastly, whether, as an acid austere medicine, they may cool, correct, and give a better consistence, in a colliquative state of the blood; seems well to deserve further inquiry, and that the result should be communicated for the public utility.

There had indeed formerly obtained a general prejudice against the use of the ordinary chalybeates in diseases of the lungs; but, at length, experience hath convinced us, not only of their safety, but usefulness, and good effects, especially when tempered with milk, in many of those cases. And moreover, it is but doing justice to our acid vitriolic waters, to acknowledge, that the empirical trials made on them by the giddy vulgar have been frequently such, as demonstrate, not only their safety, but even powerful effects in other rebellious disorders; as, particu-

larly, our Kilbrew water (one of the sharpest and most strongly saturated with martial vitriol of all these waters yet discovered), in the notable cure of an ascites, complicated with a jaundice, which I have elsewhere related; and I see no reason why physicians should not, in this as well as other cases, avail themselves of the happy success of such casual experiments.

In order, therefore, to promote a view of this kind; and, furthermore, as these vitriolic waters are better adapted for use than the ordinary chalybeates, as bearing carriage to remote places, and may be kept fit for use at all seasons of the year, and are to be preferred in medical intentions, whenever the strongest of the chalybeates are required, and can be borne; I shall here, from facts and observations made on the several waters of this sort, which have fallen under my notice, give a short sketch of their general operation and good effects, as a foundation for further improvements.

These waters, then, generally operate as an emetic or cathartic, or both; and have recommended themselves, in external and internal use, as a powerful detergent, repelling, bracing, styptic, cicatrizing, antiscorbutic, and deobstruent medicine, as hath appeared by the notable cures they have effected, not only by external use in inveterate ulcers, the itch, mange, scab, tetters eruptions, scald head, and sore eyes; but also by internal use in hot tetters eruptions, dysenteries, internal hæmorrhages, in gleets, the fluor albus, and diarrhæa, in the worms, agues, dropies, and jaundice.

Such

Such hath been the success, that has not unfrequently crowned the empirical use of these waters; which, though, in some of these cases, it might undoubtedly have been better conducted in the hands of the prudent physician, may, however, suffice to convince us, that the vitriolic waters are a branch of the *materia medica*, not to be despised nor overlooked, in the cure of many stubborn chronical diseases.

Dublin, 15 February, 1750.

XLVI. *An Account of that Part of America, which is nearest to the Land of Kamtchatka; extracted from the Description of Kamtchatka by Professor Krasnennicoff, printed at Petersburg, in two Volumes, 4to. in 1759; and translated and communicated by the Rev. D. Dumaresque, D. D. Chaplain to the English Factory at Petersburg.*

CHAPTER X.

Of America.

Read Jan. 24, 1760. **A**Lthough we have no exact and circumstantial accounts of America, which lies east of Kamtchatka, for which reason, the description of that country might be deferred to the time, that the voyage to America, at the Kamtchatka expedition, will be published; nevertheless,

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for

for the sake of regularity, and that the reader may still have some notion of all the countries, that are in the neighbourhood of Kamtchatka, we are going to communicate here, what is collected in the memoirs of Mr. Steller, in different places.

The continent of America, which now is known from 52° to 60° of north latitude, extends from the south-west to the north-east, every-where almost at an equal distance from the Kamtchadalian shores, *viz.* about 37° longitude: for the Kamtchadalian shore, also, from the Kurilian Lopatka [the shovel *] to cape Tchukotski, in a strait line (except where there are bays and capes), lies in the very same direction. So that one has grounds to infer [from thence], that those two lands were once joined, especially in those parts, where lies cape Tchukotski: for, between that and the coast, that projects, which is found at the east, directly over-against it, the distance does not exceed two degrees and a half.

Steller brings four arguments to prove this:

1. The state of the shores, which, both at Kamtchatka and in America, are ragged [broken, cragged].
2. The many capes, which advance into the sea, from 30 to 60 verstes.
3. The many islands in the sea, which separate Kamtchatka from America.
4. The situation of those islands, and the inconsiderable breadth of that sea.

As to the rest, we leave this to the consideration [or judgment] of more skilful persons: sufficient it

* The words included between the [] are added by way of illustration, or else are, for the most part, the literal translation of the Russ.

will be for us, barely to relate what was observed round about those parts.

The sea, which divides Kamtchatka from America, is full of islands, which [lying] over-against the south-west end of America, extend [as far as] towards the streights of Anian, in such an uninterrupted series [row, order], as the Kurilian islands do [as far as] towards Japan. That row of islands is found between 51 and 54° latitude, and lies directly east; and it begins not farther than 5 degrees from the Kamtchadalian shore.

Steller thinks, that Company's-land is to be found between the Kurilian and the American islands (which many doubt of), if one [setting out] from the south-west extremity of America, advances south-west: for, in his opinion, Company's-land must be the base of a triangle [which it forms] with the Kurilian and the American islands; which seems not to be destitute of foundation, if Company's-land be rightly laid down on the maps.

The American land is in a much better state, with regard to the climate, than the farthermost eastern part of Asia, though it lies near the sea, and has, every-where, high mountains, some of which are covered with perpetual snows; for that [country], when its qualities are compared to those of Asia, has, by far, the advantage. The mountains of [that part of] Asia are, every-where, ruinous and cleft [broken]; from whence they have, long since, lost their consistency, they have lost their inward warmth; upon which account, they have no good metal [of any kind]; no wood, nor herbs, grow there, except in the vallies, where is seen small [brush-] wood and
stiff

stiff herbs. On the contrary, the mountains of America are firm, and covered, on the surface, not with moss, but with fruitful earth [or mould]; and therefore, from the foot to the very top [of them], they are decked with thick and very fine trees. At the foot of them grow herbs proper to dry places, and not to marshy ones; besides that, for the most part, those [plants] are of the same largeness and appearance, both on the lower grounds, and on the very tops of the mountains; by reason, that there is every-where the same inward heat and moisture. But in Asia, there is so great a difference between them, that, of one kind of plants growing [there], one would [be apt to] make several kinds, if one did not observe a rule, which holds generally, with regard to those places [*viz.*], that in lower grounds herbs grow twice as large as those on the mountains.

In America, even the sea-shores, at 60° latitude, are woody; but in Kamtchatka, at 51° latitude, no place set with small willows and alder-trees is found nearer than 20 verstes from the sea: plantations [or woods] of birch-trees, are, for the most part, at [the distance of] 30 verstes; and, with regard to pitch-trees, on the river Kamtchatka, they are at the distance of 50 verstes, or more, from its mouth. At 62°, there is no wood at Kamtchatka.

In Steller's opinion, from the afore-mentioned latitude of America, the land extends as far as 70°, and farther; and the chief cause of the above-said growth of woods in that country is the cover and shelter it has from the west. On the other hand, the want of this [of wood] on the Kamtchadalian shores, especially on the shore of the Penshinian sea,

doubtless, comes from a sharp north wind, to which it is much exposed. That those parts, which lie from the Lopatka, farther to the north, are more woody and fruitful, is owing to cape Tchukotski, and the land that has been observed over-against it, by which those [parts] are sheltered from the sharp winds.

For this reason, also, fish comes up the rivers of America earlier than those of Kamtchatka. The 20th of July, there has been observed a great plenty of fish in those rivers; whilst at Kamtchatka, it is then but the beginning of an abundant fishery.

Of berries, they saw there an unknown kind of raspberries, which bore berries of an extraordinary bigness and taste. As to the rest, there grow in that country black-berries [with several other kinds of berries, called in Rus, *jimolost*, *golubitsa*, *brusnitsa*, and *shik/ka*], in as great plenty as at Kamtchatka.

There are creatures enough, good for the support of the inhabitants of those parts; particularly seals, sea-dogs, sea-beavers, whales, * *canis carcharias*, *marmottes* [*marmotta minor*], and red and black foxes, which are not so wild as in other places, possibly because they are not much chased [hunted].

Of known birds, they saw there magpies, ravens, sea-mews [† *urili*], sea-ravens, swans, wild ducks, jackdaws, woodcocks, ‡ Greenland pigeons, and

* In Rus, *akul*, or *mokoia*; in bigness, it is inferior to the whale; and it is like it in this, that it casts no spawn, but brings forth young; upon which account, some reckon it a species of whale. Descript. p. 308. 1st vol.

† *Uril*, *corvus aquaticus maximus*, *cristaceus*, *periophthalmiis cinnabarinis*, *postea candidis*.

‡ *Columba Groenlandica*, *Batavorum*, *kaiover*, vel *kaicur*.

* *mitcha-*

* *mitcbagatki*, otherwise called northern ducks. But, of unknown birds, they observed more than ten sorts, which it was not difficult to distinguish from European birds, by the liveliness of their colours.

With regard to the inhabitants of those parts, they are such a wild people, as the Koriaki and Tchuktchi. As to their persons, they are well set, broad and strong shouldered. The hair of their head is black, and strait, and they wear it loose. Their face is tawny [brown], and flat as a plate; their nose is flat, but not very broad; their eyes are as black as jet; their lips thick; their beard small; and their neck short.

They wear shirts with sleeves, which reach lower than the knee; and they tie them up, with thongs of leather, below the belly. Their breeches and boots [which are made] of the skins of seals, and dyed with alder, much resemble the Kamtchadalian. They carry, at their girdles, iron knives, with handles, such as [are those] of our boors. Their hats are platted [matted] of herbs, as with the Kamtchadalians, without a [rising] top, in the shape of an umbrella; they are dyed in green, and in black, with falcon's feathers, in the fore-part, or with [some] herb, combed, as if it were a plume of feathers, such as the Americans use about Brasil. They live upon fish, sea animals, and the sweet-herb, which they prepare after the Kamtchatka manner. Besides this, it has been observed, that they have also the bark of poplar,

* *Mitcbagatka*, alca monachroa fulcis tribus, circo duplici utrinque dependente. *Anas arctica cirrata*.

or of the pine-tree, dried, which, in case of necessity, is made use of as food, not only at Kamtchatka, but likewise throughout all Siberia, and even in Russia itself, as far as Viatka; also sea weeds made up into bundles, which, in look and in strength, are like thongs of raw leather. They are unacquainted with spirituous liquors and tobacco; a sure proof, that, hitherto, they have had no communication with the Europeans.

They reckon it an extraordinary ornament, to bore, in several places, the lower parts of the cheeks, near the mouth; and in [the holes] they set some stones and bones. Some wear, at their nostrils, slate pencils, about four inches long; some wear a bone of that bigness, under the lower lip; and others a like bone on the forehead.

The nation, that lives in the islands round about cape Tchukotski, and frequents the Tchutchi, is, certainly, of the same origin with those people: for with them also it is thought an ornament [thus] to inlay [ingraft] bones.

Major Paulutskoi, deceased, after a battle, which he once fought against the Tchutchi, found, among the dead bodies of the Tchutchi, two men of that nation, each of whom had two teeth of a sea-horse under the nose, set in holes made on purpose: for which reason, the inhabitants of that country call them *Zubatui* [toothed]. As the prisoners reported, these [men] did not come to the assistance of the Tchutchi, but to see how they [used to] fight with the Russians.

From this, it may be inferred, that the Tchutchi converse with them, either in the same language, or, at least, in languages of so great affinity, that they

can understand one another, without an interpreter; consequently, their language has no small resemblance with that of the Koriaki: for the Tchukotchian comes from the Koriatskian [language], and differs from it only in the dialect: nevertheless, the Koriatskian interpreters can speak with them, without any sort of difficulty. With regard to what Mr. Steller writes, that not one of our interpreters could understand the American language, possibly, that comes from the great difference in the dialect, or from a difference of pronunciation; which is observed, not only among the wild inhabitants of Kamtchatka, but also among the European nations, in different provinces. In Kamtchatka, there is hardly any small * *ostrog*, but what the speech there differs [somewhat] from that of another that lies nearest. As for those small *ostrogs*, which are at some hundreds of verstes from one another, those can no longer understand each other, without [some] trouble.

The following remarkable resemblances between the American and the Kamtchadalian nations, have been observed:

1. That the Americans resemble the Kamtchadales in the face.
2. That they eat the sweet-herb, after the same manner as the Kamtchadales; [a thing] which never was observed any-where else.

* *Ostrojka*, a small *ostrog*, is a place fenced and fortified with a pallisade, made of trees, fixed perpendicularly in the ground, and cut sharp at the top: sometimes they are beams laid over each other. *Ostrui*, in Russ, signifies sharp.

3. That

3. That they make use of a * wooden machine [instead of a tinder-box], to light fire with.

4. That, from many tokens, it is conjectured, that they use axes made of stones, or of bones: and it is not without foundation, Mr. Steller thinks, that the Americans had once a communication with the people of Kamtchatka.

5. That their cloaths and their hats do not differ from the Kamtchadalian.

6. That they dye the skins with alder, after the Kamtchatka manner.

Which marks shew it to be very possible, that they came from the same race. This very thing, he rightly judges, may help also to solve that question, " Whence came the inhabitants of America?" For though we should suppose, that America and Asia were never joined; nevertheless, considering the nearness of those two parts of the world at the north, no one can say, that it was impracticable for people from Asia to go over to settle in America; especially, as there are islands enough, and at so small a distance, which might facilitate not a little such a passage [in order to settle].

Their armour for war is a bow and arrows. What kind of a bow it is, we cannot say, as it did not happen [to our people] to see any; but their arrows are much longer than the Kamtchadalian, and greatly resemble the Tungusian and Tartarian [arrows]. Those, which came in the way of our people, were dyed black, and planed so smooth, that

* See Description of Kamtchatka.

they left no room to doubt of [the Americans] having also iron tools.

The Americans sail upon the sea in canoes made of skins, in the same manner as the Koriaki and the Tchutchi. * Their canoes are about two fathoms [14 feet] long, and about two feet high. The fore-part of them is sharp; and they are flat-bottomed. Their inward frame consists of sticks, which are linked together at both ends, and in the middle are pressed outwards, in a rounding [a belly], with cross-sticks [which keep the sides at a proper distance]. The skins, which they are covered with, all around, seem to be those of sea-dogs, dyed of a cherry colour. The place where the Americans sit is round, about two arshines [4 feet 8 inches] from the poop; there is sowed upon it the stomach [of some great fish], which one may gather and loosen as a purse, with the help of thongs of leather, passed through small holes at the edge. An American, sitting in that place, stretches his legs, and gathers round him the stomach [above-mentioned], that water may not fall into the canoe. With one oar, some fathoms long, they row on both sides alternately, with such a progressive force, that contrary winds are but a small hindrance to them; and with so much safety, that they are not afraid to go upon the water, even whilst the sea rises in terrible surges. On the contrary, they look with some terror upon our larger vessels, when they are tossed, and advise those, who sail in them, to beware, lest their vessels should be overset. This

* See the two figures, in 1st vol. of the Description of Kamtchatka, over against page 128.

happened

happened to the boat Gabriel, which, some years since, was going to cape Tchukotski. As to the rest, their canoes are so light, that they carry them with one hand.

When the Americans see [upon their coasts] people, whom they do not know, they row towards them, and then make a long speech : but it is not certain, whether this is by way of a spell [or charm], or some [particular] ceremony used at the reception of strangers : for both the one and the other are in use among the Kurilians. But before they draw near, they paint their cheeks with black lead, and stuff their nostrils with [some] herb.

When they have guests, they appear friendly ; they like to converse with them, and that in an amicable manner, without taking off their eyes from them : they treat them with great submission, and present them with the fat of whales, and with black lead, with which they used to besmear their cheeks, as was before observed ; doubtless from a notion, that such things are as agreeable to others, as they are to themselves.

With regard to the navigation about those parts, it is safe [enough] in spring and in summer ; but in autumn it is so dangerous, that hardly a day passes, but one has reason to fear being shipwrecked : for they [the Russians sent upon the sea expedition] experienced such a violence of winds and storms, that even persons, who had served forty years at sea, assured, with an oath, that they never saw such in their lives.

The marks, by which they observe there [in that sea] that land is near, are, particularly, the following
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considerable ones: 1. When there appears a great quantity of different kinds of the [so called] sea-cabbage, swimming upon the water. 2. When one sees the herb, of which, at Kamtchatka, they platt cloaks, matts, and bags; for it grows only on the sea-shores. 3. When there begin to appear, at sea, flights of sea-mews, as well as droves of sea animals, such as sea-dogs, and the like: for though sea-dogs have a hole open at the heart, which is called *foramen ovale*, and a duct called *ductus arteriosus Botalli*, and, upon that account, may remain long under water, and, consequently, go to some distance from the shore, without danger, inasmuch as they can, at a greater depth, find food proper for them: nevertheless, it has been observed, that they seldom go farther than ten [German] miles from the shore.

The surest sign, that land is near, is, when there are seen Kamtchatka beavers, which live only upon crabs, and, from the make of their heart, cannot be under water above two minutes [at a time]; consequently, they cannot get food at the depth of 100 fathoms, or indeed at a much lesser depth; upon which account, they also breed always near the shore.

It remains still to speak of some islands nearest to Kamtchatka, which are not found in a strait line with the above-mentioned, but north of them; especially of Berings island, which now is so well known to the inhabitants of Kamtchatka, that many of them go thither, to catch sea-beavers, and the like sea animals.

That island extends from the south-east to the north-west, between 55 and 60°. Its north-east end, which lies almost directly over-against the mouth of
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the river Kamtchatka, is at about two degrees distance from the eastern shore of Kamtchatka: and its south-east end is about three degrees from cape Kronotski. This island is 165 verstes long; but its breadth is unequal. From the south-east end to a rock, which hangs perpendicularly over the sea, and is at 14 verstes distance from that end, the breadth of the island is from 3 to 4 verstes: from that steep rock to Suiputchei bay, it is 5 verstes: from Suiputchei bay to Beaver's steep rock, it is 6 verstes: at Whale's-stream, it is 5 verstes; but from thence farther on, it grows gradually broader. Its greatest breadth [*viz.*], 23 verstes, is over-against the north cape, which lies 115 verstes from the above-mentioned end.

In general, it may be said, that the length of that island is so disproportioned to its breadth, that our author doubts, whether there can be, in other parts of the world, any islands of such a shape; at least, he never heard nor read of any such: and he adds, that the islands, which they saw about America, and all the rows of them, lying to the east, have the like proportion.

This island consists of a ridge of rocks, which is divided by many vallies, that stretch to the north and to the south. Its mountains are so high, that, in clear weather, one may see them from about half the distance between the island and Kamtchatka. The inhabitants of Kamtchatka, of old times, thought, that there must be [some] land over-against the mouth of the river Kamtchatka, by reason that [the sky] appeared there always cloudy, though it were never so clear every-where else about the horizon.

The

The highest mountains [of the island] do not measure above 2 verstes in a perpendicular.

[Here follows a description of this barren rocky island, of which the following four pages are not to our present purpose. Then, at page 136, the account proceeds thus:]

The south-west side of the island is of a quite different nature [from the other, as to access]: for tho' the shore [there] is more rocky and craggy, yet there are two places, by which, in flat-bottomed boats, such as are the * *tscherbotui*, one may not only land on the shore, but even advance as far as a lake, by the streams [that flow from it]. The first of these places is at 50 verstes, and the other at 115, from the south-east end of the island.

This last place is very remarkable from the sea; for the land there goes rounding from the north to the west; and, at the very promontory, there runs a stream, which is the largest of any in that island; and, when the water is high, its depth is not less than 7 feet. It runs from a great lake, which lies a verste and a half from its mouth: and because that stream grows deeper, the farther it is from the sea, therefore one may conveniently go upon it in boats as far as the lake: and upon the lake there is a safe station; for it is surrounded with rocky mountains, as with a wall, and sheltered from all winds. The chief mark, by which one may know this stream from the sea, is an island, which is about 7 verstes in circumference, and lies to the south at 7 verstes distance from the mouth of the stream. The shore

* Large canoes, or boats, somewhat resembling ferry-boats.

from thence towards the west, is sandy and low for 5 verstes. Round the shores, there are no rocks under water; which one may know from thence, because there are no * eddies [breakers].

From the highest rocks of that island, one sees the following lands: at the south, two islands, one of which measures about 7 verstes in circumference, as was observed before; but the other is over-against the very end of Berings island, at the south-west: it consists of two high and cleft rocks, of about three verstes in circumference, and is at 14 verstes distance from Berings island.

From the north-east end of Berings island, in clear weather, one may see, to the north-east, very high mountains, covered with snow, and their distance may be computed at 100 or 140 verstes. Those mountains our author thought, with better grounds, to be a cape of the continent of America, than an island: 1. Because those mountains [allowing for] considering their distance, were higher than the mountains on the [neighbouring] islands. 2. Because that, at a like distance towards the east, one observes plainly, from the island, such like white mountains, from the height and extent of which all judged that it was the continent.

From the south-east end of Berings island, they saw, to the south-east also, another island, but not very clearly: it seemed to lie between Berings island and [some] low part of the continent.

From the west and the south-west sides, it was observed, that, even in clear weather, there is a per-

* In German, *brennung*.

petual fog above [higher up than] the mouth of the river Kamtchatka; and from thence, in some measure, they came to know the inconsiderable distance of the land of Kamtchatka from Berings island.

North of the so often mentioned Berings island, there is another island, in length from 80 to 100 verstes, which lies parallel to it, *i. e.* from the south-east to the north-west. The fireights between these two islands, at the north-west, measure 20 verstes, and at the south-east about 40. The mountains upon it are lower than the ridge [of mountains] in Berings island. At both ends of it, there are, in the sea, many rocks at [low] water mark, and perpendicular rocks like pillars.

With regard to the weather, it differs from that at Kamtchatka only in this, that it is more severe and sharp: for the island has no shelter from any quarter; and, besides that, it is narrow, and without woods.

Moreover, the force of the winds increases to such a degree, in those deep and narrow vallies, that one can scarce stand upon one's legs. In February and April months, were observed the sharpest winds, which blew from the south-east and from the north-west. In the former case, the weather was clear, but tolerable; but in the latter case, it was clear, indeed, but extremely cold.

The highest rising of the water happened in the beginning of February month, during north-west winds: the other inundation was in the middle of May, occasioned by great rains, and by the sudden thawing of the snows. Nevertheless, those floods were moderate, in comparison with those, of which there [still] remained undoubted marks: for there
have

have been carried many trees, and whole skeletons of sea animals, to the height of 30 fathoms, or more, above the surface of the sea [above the common water mark, or level]: from which our author judges, that, in the year 1737, there happened, likewise, here [in this island] such an inundation as that at Kamtchatka.

Earthquakes happen [here] several times in the year. The most violent, that was observed, was in the beginning of February, which, during a westerly wind, lasted exactly six minutes; and before it was heard a noise, and a strong wind, under ground, with a hissing, which went from south to north.

Among mineral things, which are found in that island, one may reckon, as the most remarkable, the fine waters, which, upon account of their pureness and lightness, are very wholesome: and this virtue of them was observed upon sick people, with advantage, and the desired satisfaction. With regard to the plentifulness of them, there is not a valley but what has a stream running through it; and the number of them all together exceeds sixty; among which there are some, which are from 8 to 12 fathoms broad; and some are two, and some even 5 fathoms deep, when the water is high; but there are few such, and the greater part of them is extremely shallow at the mouth; because that they have a very rapid course, on account of the steep slopes of the vallies, and that near the sea they divide into many rivulets.

Explication of some Words, which occur in the Reise-Beschreibung, &c.

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6. *Promyschleni* properly signifies those, that make a trade or profession of hunting in Siberia.
7. *Gostinna sotna*, the merchants of the first class in Russia.
- Ibid.* *Semeon Iwanow sin Deschnew*. When the Russians speak of a man of quality, they add to his Christian name that of his father; *ex. gr.* *Simon Ivanowitsch Deschneff*; but if of a common man, they say, as here, *Simon Ivanoff sin* (*i. e.* son of) *Deschneff*, Simon the son of John Deschneff.
8. *Cofacke*. The Cofacks are properly the soldiers, of whom consists the Ukrain militia, or that of the neighbourhood of the Don. These last began the conquest of Siberia. Their posterity is still employed in raising the taxes laid on the Siberian nations, which have submitted to the Russian empire.
- Ibid.* *Zimmer*, is a bundle of furs, consisting of 40 skins.
11. *Simowie* [*zimovie*], properly, is an habitation, or quarters, for the winter; a hut or huts, built hastily, in some desert place, in order to spend a winter there. When the number of those huts has so increased, as to become a village, it has often still retained the old name of *zimovie*. *Zima*, in Russian, is winter.

Ostrog.

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11. *Ostrog*. See page 484 above.
14. *Narte*, a kind of small fledge, very light, drawn by dogs. See the representation of it in the Description of Kamtchatka.
19. *Werchnei*, *nischnei*, *serednie*, *bolsche*; these words signify, high, low, middle, and greater; as *werchnei Ostrog*, the upper Ostrog, &c.
24. *Swaetoi*, signifies holy, saint; as *Swaetoi nos*, cape Holy.
26. *Piaetidesaetnifs*, an officer, who has 50 men under his command, and who may be called a lieutenant. In Russ, *pietderiat* is fifty.
27. *Woerwood* is the governor of a town, of a district, or of a province.
33. *Stolnik* was an officer, formerly, who set the dishes upon the tsar's table. *Stol*, a table.
- Ibid.* *Knjaes*, a prince.
34. *Dworaen*, a gentleman.
- Ibid.* *Ust*, mouth. *Ust Jana*, the name of a place situated at or near the mouth of the river Jana.
- Ibid.* *Nos*, cape, promontory, nefs, neese. The proper signification of the word is nose.
40. *Schikiti*, sewed, from *schit*, to sew.
41. *Werste*, an itinerary measure in Russia, consisting of 500 *sajens*, or fathoms, each of which makes 7 feet English.

Sin

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42. *Sin bojarzkoi*, a title which is conferred, in Siberia, upon low people, in order to raise them to a degree nearer to that of a noble or a *gentilhomme*.

59. *Vielfrass*, a German word, which signifies a glutton. It is a wild beast of a small size, about the bigness of a fox, but more resembling a bear. This creature, called in French, *glouton*, is extremely voracious, from whence it had its name.

Ibid. *Steinfuchs*, is a kind of fox.

86. *Baidar*, canoe.

Ibid. *Nessel*, a nettle, in general, in German; here a kind of nettles, of which a particular stuff is made.

Ibid. } *Levkashenuiu posudu*, signifies a vessel or pot
linea } made of *levkas*, a kind of alabaster. Here
penult- } it probably was intended to express China
tima. } ware, or porcelaine.

90. *Rietgras*, a German word, signifying the same as bamboo.

121. *Judomskoi krest*, Judoma's-cross, the name of a village near the river Judoma, in a place on which was erected a cross, when it was first discovered.

Ibid. *Belskoi perewoss*, the ferry of Bela.

Ibid. }
linea } *Jiujnoi*, southerly, to the south.
penult. }

143. *Krepost*, signifies a castle, or a fort.

Ibid. *Sawod*, a manufactory, or fabrique.

Ibid. *Woskresenskei*, belonging to the resurrection.

Plct-

Page

165. *Plotbischtsche*, the place where are built the
(*plotui*) floats, or small boats.

181. *Buxiren*, a German word, which signifies to
tow, to take in tow.

182. *Dannen* (or *tannen*) *knospe*, the buds of fir-
trees.

151. *Buikowskoi muis*, *parvum promontorium bo-
vinum*.

251. }
linea } Read *korova morskai*a, the sea-cow.
antep. }

250. }
lin. 6. } Read *viporotki*.

249. }
lin. 16. } Read *kotui morskije*, sea-cats.

Guba, a gulf, or bay.

Matrofs, a sailor.

Nova Zemla, New Land, *Terra Nova*.

Gorodock, a small town, *oppidulum*.

Sloboda. See the introduction to the *Atlas
Russicus*, in which many Russian names of
places, &c. are explained.

XLVII. *Remarks on the Mutations of the Stars*; by Tho. Barker, Esq; of Lyndon, in Rutland: Communicated by the Rev. W. Stukeley, M. D. F. R. S.

Read Jan. 31, 1760. **I**T is well known there have been several alterations among the fixed stars: for instance, Ptolemy's *ultima fluvii*, a first magnitude star, is in Dr. Halley's catalogue of the southern constellations only a third magnitude: and in much less time, the δ of the Great Bear, which Bayer seems to have judged just of the same size with the other six, is grown far duller than any of them. Some stars also have quite disappeared, while again new ones, not seen before, have been discovered: and there are others periodically larger and smaller. Two very remarkably bright, yet short-lived, stars, have been also seen, one in Cassiopeia, the other in Serpentarius; which breaking out, at once, with greater lustre than any other fixed star, gradually faded, and changing to different colours, in about a year and half were no longer visible. But, I think, no one has yet remarked, that any lasting star was of a different colour in different ages: Greaves, on the contrary, takes notice, that the colours of the stars and planets are the same now as the antients observed; which is, I believe, very true in general: for Ptolemy, in his catalogue of stars, says, Arcturus, Aldebaran, Pollux, Cor Scorpii, and Orion's Shoulder (with another to be mentioned presently), are *ὑποκίττοι*, reddish: and the five here mentioned are still
of

of that colour, and, I think, the only considerable stars which are so.

But, to this rule there seems to be one exception, and that in a remarkable star: for old authors mention the Dog star, which is now white, and not at all inclined to redness, as being then very much so; as in the following places:

Τοιος και φρερος αειρομενω ὑπο νωτω
Φαινεται αμφοτεροισι κυων ὑπο ποσσι βεβηκως
Ποικιλος ————— Αρατε φαινομενα. 326.

Which Cicero thus turns—See Gruter's Cicero, IV. 359.

Namque pedes subter rutilo cum lumine claret
Fervidus ille Canis stellarum luce refulgens.
Seu rubra Canicula findet
Infantes statuas ————— *Hor. Sat. II. 5, 39.*

Acrior fit Caniculæ rubor, Martis remissior Jovis
nullus. *Seneca Quæst. Nat. I. 1.*

Ὁ ἐν τῷ σχηματι λαμπροτατος καθεμένος κυων ὑπο-
κιρρος. *Ptolemy. Κυνος αστερισμος.*

Ποικιλος, in the quotation from Aratus, does not expressly mean red; but is always used of something shewy, glittering as gold, various-coloured, &c. as in the following places.

Τειχεα ποικελα χαλκω. *Homer's Il. v. 181.*
Παρδαλη μεν πρωτα μεταφρενον' ευρι καλυψε
Ποικιλη ————— *Il. κ. 30.*
Ὅς καλλιζος ἐην ποικιλμασιν ————— *Od. ο. 107.*
Βητην εἰς Ὀδυσῆα δαιφρονα ποικιλομητην. *Od. χ. 202.*
VOL. LI. T t t Aratus

Aratus therefore, I think, shews at least, that the Dog star was not then of the same colour as other stars: and, as Cicero turns it *ruilus*, it appears he either understood the word to mean red, or knowing by his own view it was so, thought it the proper interpretation; for *rutilus* is used of what is reddish, and often of the red glare of a fire, or the dawn, as below:

—rutilum vomit ille cruorem. *Ovid. Met. V. 33.*

Promissæ et rutilatæ comæ. *Livy. XXXVIII. 17.*

Arma inter nubem, cœli in regione serena.

Virg. Æneid. VIII. 528.

Per sudum rutilare vident,

Sin maculæ incipient rutilo immiscerier igni.

Georg. I. 454.

Auroram rutilare procul cerno.

Varro, de Ling. Lat. VI. 5.

Rubra, in Horace, will, I think, bear no other sense than red, or else it is the heat he there chiefly speaks of: and though, I think, Latin authors confound *Canicula*, some using the word for *Sirius*, others for *Procyon*; yet it plainly appears, that *Sirius* is here meant, since Horace always calls it *Canicula*, and never uses the word *Sirius*: but Aratus and Ptolemy leave no room to doubt what star it was, being expressly speaking about the Dog star.

Seneca says, the redness was so strong as to exceed that of Mars, to which no star now approaches. None of the notes on Seneca clear up this matter: Fromondus, indeed, observed the place, and declared his astonishment at it; but does not attempt to

to solve the difficulty. Ptolemy's is, however, the most undeniable evidence, who, when directly describing the stars, and particularly mentioning the Dog star, says, expressly, it was of the same colour as Cor Scorpii, and the other stars, which are still red; so that I do not see how his evidence can be disputed.

There is, however, one objection to what I have said, but I, think, not an unanswerable one; which is, that, at first sight, Hyginus seems to call Sirius white: but since, if so, he contradicts the other authors I have above quoted to prove it red, and, because he there says something I do not well understand, I shall quote the whole.

Hygini Poetic Astron. II. 35.

Canis habet in lingua stellam unam, quæ ipsa Canis appellatur; in capite autem alteram, quam Isis suo nomine statuiffe existimatur, et Sirion appellasse propter flammæ candorem; quod ejusmodi sit, ut præter cæteras lucere videatur, itaque quo magis eam cognoscerent, Sirion appellasse.

He again distinguishes these two stars, *lib. III. 34.*

Canis habet in lingua stellam unam, quæ Canis appellatur, in capite autem alteram, quam nonnulli Sirion appellant, de quo prius diximus.

Of two stars in the Dog's head, Isis and Sirius, Eratosthenes also speaks:

Καταστερισμος λγ. Κυων — Εχει δε αστερας, επι
μεν της κεφαλης α' ος Ισις λεγεται, της γλοττης α' ον και
Σειρων καλεσι μεγας δε εστ' η λαμπρος, τες δε τοις τες
T t t 2 αστερας

αἰθέρας οἱ ἀστρολόγοι σείριος καλεῖσι· διὰ τὴν τῆς φλόγος
κίνησιν.

Hyginus, in distinguishing Canis from Sirius as two different stars, seems, to me, to contradict all other writers, who speak of them as one, except, perhaps, two or three latter ones, who directly quote Hyginus's words. Sirius, or Canis, the brightest star in the heavens, is that, which Ptolemy calls in the mouth; Eratosthenes and Hyginus, in the tongue: but whether Bayer γ, which Flamsteed calls a third magnitude star, Ptolemy only a fourth, was in more antient times larger, I will not pretend to say; since, Eratosthenes and Hyginus both speak of two stars in the Dog's head, as thought worthy of particular names. If, in Hyginus, *flammæ candorem* means the whiteness of its light, as *candor* often does, he expressly contradicts what I have quoted above from others; yet still I think Ptolemy's authority is greater than that of Hyginus. But that *candor* is also used for innocence, beauty, brightness, &c. take the following examples.

Bis fenis equis candore eximio trahentibus. *Suet. Cæs. Octav. 94.*

Si tamen ille prior, quo me fine crimine gessi
Candor—— *Ovid. Epist. IV. 31.*

—— formæ nisi candor. *Metam. I. 743.*

Candore noto reddas judicium peto.
Phædrus. III. Prol. 64.

Pendebant ex auribus insignes candore et magnitudine Lapilli. *Quint. Curt. IX. 4.*

Ut cum videmus speciem primum, candoremque
cœli. *Cic. Tusc. Quæst. I. 28.*

Solis candor illustrior quam ullus ignis. *De Nat.
Deor. II. 15.*

In the second or third last quotation, *candor* is used in the same sense as in Hyginus, for brightness, without regard to colour; for so, I think, he must be understood, not only to avoid contradiction between him and Ptolemy, but from the name *Sirius*, which it could not be called from its whiteness, Σειριος bearing no relation to that; but to brightness, heat, or dryness; all which the antients speak of, as properties of the Dog star. Again, it is brightness, wherein it excels all other stars, and not in whiteness; for Orion's foot and others are as white, but there is none so bright as the Dog star. All this is said, on supposition there was but one remarkable star in the Dog's head, that in the mouth: for if there were two, as Hyginus says, we are not here concerned with either the brightness or colour of his *Sirion*, which was in the head, as it certainly faded before Ptolemy's time, who mentions only one, that in the mouth, and which, he says, was then red, but is now white.

To conclude the whole; however remarkable and without precedent it may be, that so noted and lasting a star as the Great Dog should have changed its colour, yet as at least five different writers affirm it, some so expressly, and where their subject required them to speak particularly about it, it appears to me to have been certainly the case. If, however, any one, startled at the strangeness of the thing, thinks
the

the evidence I have brought insufficient to prove it, he is desired to invalidate what I have here said, by a deduction of fresh evidence, and to account for these several expressions in old authors, which seem to prove, that such a change has really happened.

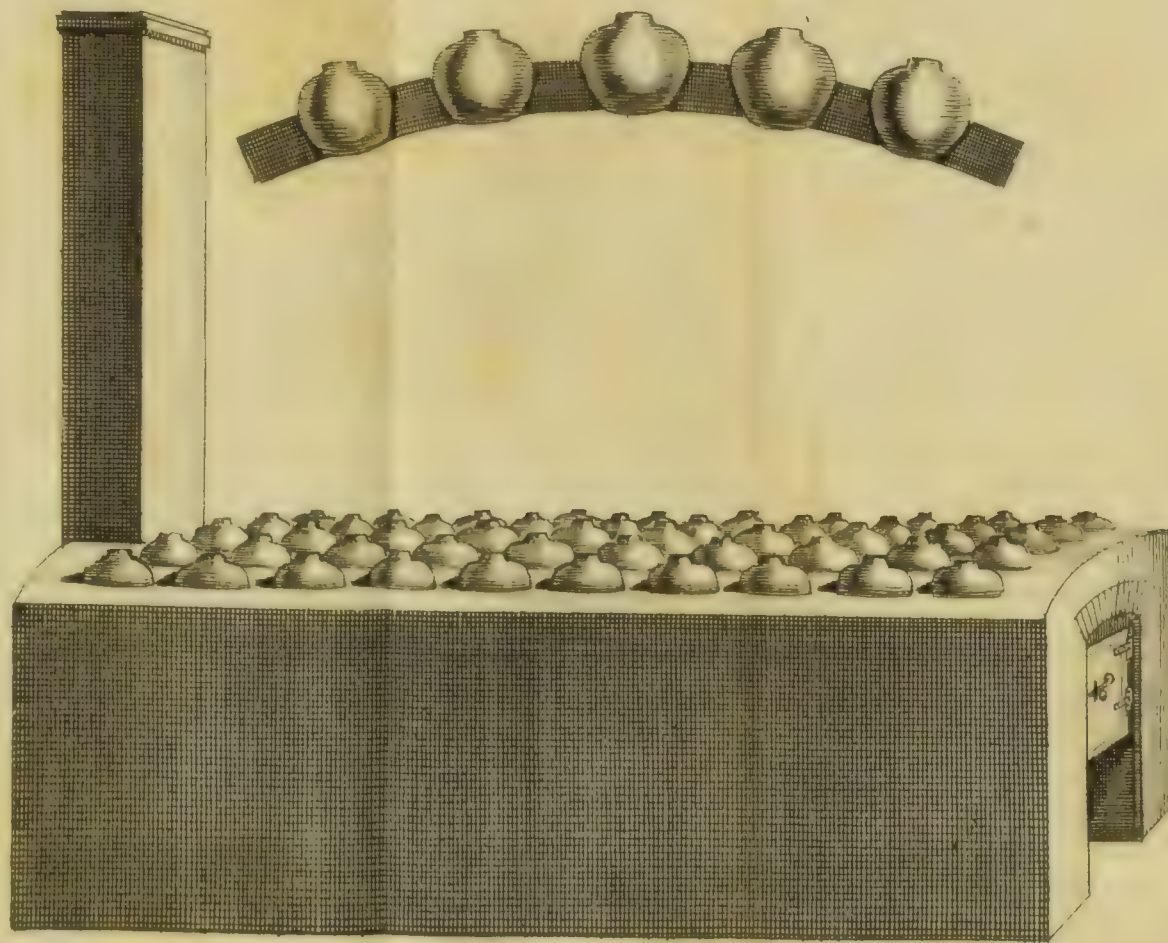
XLVIII. *The Method of making Sal Ammoniac in Egypt; as communicated by Dr. Linnæus, from his Pupil Dr. Hasselquist, who had been lately in those Parts: By John Ellis, Esq; F. R. S.*

Read Jan. 31, 1760. **S**AL Ammoniac is made from the foot arising from the burnt dung of four-footed animals, that feed only on vegetables.

This dung is collected in the four first months of the year, when all their cattle, such as oxen, cows, buffaloes, camels, sheep, goats, horses, and asses, feed on fresh spring grass, which, in Egypt, is a kind of trefoil, or clover: for when they are obliged to feed their cattle on hay, and their camels on bruised date kernels, their excrements are not fit for this purpose; but when they feed on grass, the poor people of Egypt are very careful to collect the dung quite fresh, and, for that purpose, follow the cattle all day long, in order to collect it as it falls from them; and, if it is too moist, they mix it with chaff, stubble, short straw, or dust, and make it up in the form of cakes, about the same size and shape as it lies on the ground.

Then





*A Plan of the subliming Furnace or Oven for making Sal Ammoniac in Egypt
taken from the description here given.*

Then they fix it to a wall to dry, till it is fit to be burnt.

For want of wood, which none but the rich in Egypt can afford to buy, they burn this dung through the whole country, and sell a vast quantity of it to the salt-makers.

The excrements of the camel are not found at all preferable to any other; and its urine is never used for this purpose, although generally reported so by authors.

The salt-workers pretend, that the human excrements, and those of goats and sheep, are preferable to any other.

The months of March and April is the only time they make the salt.

Sal ammoniac is made in the following manner:

They build an oblong oven, about as long again as broad, of brick and moist dung, of such a size, that the outside, or flat part of the top of the arch, may hold fifty glass vessels, ten in length, and five in breadth, each vessel having a cavity left for it in the brick-work of the arch. *See Tab. XI.*

These glass vessels are globular, with a neck an inch long, and two inches wide.

These vessels are of different sizes, in different salt-works, containing from a gallon to two gallons; but, in general, are about 18 inches diameter.

They coat each vessel over with a fine clay (which they find in the Nile), and afterwards with straw; they then fill them two-thirds full of foot, and put them into their holes on the top of the oven.

They make the fire gentle at first, and use the afore-mentioned dried dung for the fuel; they increase

crease the heat gradually, till they bring it to the highest degree, which the workmen call hell-fire, and continue it so for three days and three nights together.

When the heat is come to its due degree, the smoke shews itself, with a sourish smell, that is not unpleasant; and, in a little time, the salt sticks to the glasses, and covers the whole opening. The salt continues subliming, till the above-mentioned time is expired; then they break the glasses, and take out the salt, just in the same form, and of the same substance, that it is sent all over Europe.

At each salt-work, they have a glass furnace, to melt the old glasses, and make new ones.

XLIX. *Montium quorundam præaltorum, magna ligni fossilis copia quasi infarctorum, brevis descriptio* Sam. Christ. Hollmanni, *Philosophiæ Professor. Goettingensis, et S. R. Sodalis.*

Read Feb. 14, 1760. **S**UNT in Hassiæ et principatus Goettingensis confiniis montes quidam præalti, qui tanta *ligni fossilis* copia abundant, ut fidem fere superet. Horum alter non procul Mundenfi oppido, circa Werram Fuldamque confluentes posito, versus meridionalem plagam ad tantam altitudinem assurgit, ut mercurius barometricus in eodem ad 1" 25''' pedis Lond. ab illo gradu descendat, quem ad Werræ Fuldæque, ipsiusque adeo ex iisdem oriundi Visurgis, ripas habet: unde ex repetitis in
utroque

utroque loco observationibus, mercuriique adeo altitudine media, utrobique, quam maxime fieri potuit, determinata, montis, de quo nunc pluribus dicendum est, altitudinem ad 1150 circiter pedes Parisinos revocare licebit. In hujus montis summo fere vertice planities quædam, si ita appellare fas est, reperitur, quæ ad sexcentarum circiter orgyrum latitudinem, septingentarum vero longitudinem, excurrit, sub cujus superficie, ad 5, 10, 15 pedum profunditatem ingentia lignorum fossilium strata reperiuntur, quæ in illis locis, ubi ligna ista fossilia ad suos, quibus destinata erant, usus eruebantur, 20 circiter pedum altitudinem tunc temporis, quando ante paucos annos ibidem aderam, habebant. Quoniam stratorum vero istorum fundus, cui insistebant, lapideus, *die sohla* metallifossiores nostri vocant, dimidium circiter pedem crassus solum erat, Dominusque fundi rescire cupiebat, num infra fundum istum lapideum ejusdem ligni copia aliqua forsan superesset; terebra quadam, quali ad exploranda montium viscera uti metallifossiores nonnunquam solent, periculum factum est, repertumque, infra fundum istum lapideam ejusdem ligni copiam ad 30 circiter pedum profunditatem adhuc superesse, neque ad novum tamen ejusdem fundum perventum adhuc erat; eandemque ligni istius fossilis altitudinem in aliis etiam ejusdem montis locis, ubi inquirere in easdem licuit, reperiri, mihi confirmatum est. Hæc ligna in stratis suis ita sibi incumbunt, ut in sectione ipsorum perpendiculari, qualis tunc temporis, quum ibidem adessem, ad fundum ipsorum lapideum usque ad 20 circiter et plurimum pedum latitudinem facta erat, ligna ipsa lapidum, in lapicidinis sibi incumbentium, situmque horizonti parallelum servan-

tium, formam fere referens, adeo arcte etiam sibi invicem quasi impacta sint, ut vix terræ interlabentis quicquam observare liceat: unde difficillimum cognitu evadit, num *arbores integræ*, an *arborum* jam fissarum divisarumque *partes* solum, fuerint, ex quibus ista enata sint. Temporis etiam, quo hunc situm procul dubio jam obtinuerunt, longinquitate, terræque simul incumbentis et aquæ interlabentis humiditate, adeo fragilia facta sunt, ut non in naturali suo situ solum rimarum, versus omnes fere directiones excurrentium, plena sint, sed loco etiam suo mota in majores facile minoresve portiones dilabantur. Color omnium horum lignorum a fusco ad nigrum fere tendit, quo etiam terra illa pauca passim intermixta non solum, sed eidem etiam incumbens, ad pedis unius et ultra altitudinem, simul tincta est: unde et hac, licet caute remota, ne in extrema quidem superficie justam lignorum istorum faciem, imo ne situs quidem directionem veram, cognoscere satis distincte licet. Terra enim ista humida ligni semiputridi rimæ et intervalla omnia adeo oblita repletaque sunt, ut visum omnem fallant confundantque. Quæ huic vero terræ incumbit, humus est intermixto sabulo, in qua optimæ passim argillæ bolique, alibi vero arenæ etiam subtilissimæ, varique coloratæ, reperire strata licet. Ex his ipsis terrarum circumjacentium stratis vero novus quasi adhuc assurgit in summo cacuminis vertice mons, quingentorum et quod excurrit passuum ambitum circiter habens, lapidibus ingentibus fere totus constratus, arboribusque præterea proceris confitus, sub quo ipso quoque lignorum istorum strata quædam adhuc excurrunt.

Ligna,

Ligna, quæ ex stratis hisce eruuntur, limo humoque interlabente ita plerumque inquinata et obducta sunt, ut a gleba quadam terrestri primo adspectu non multum differant: unde et a fossoribus et operariis, qui iisdem eruendis adhibentur, *carbones fossiles* plerumque appellantur. Quando aqua vero leviter abluta et purgata sunt, aerique ficciori per breve temporis spatium exposita, naturam indolemque suam genuinam adeo clare statim produnt, ut vel imperitissimus et stupidissimus quisque ne per momentum quidem temporis dubitare possit, num ligna vere talia sint, an minus. Quod nudo vero oculo in iisdem ita jam agnoscitur, illud ipsum vel leviter armato magis adhuc patescit: siquidem et *tubuli* lignorum *perpendicularares*, cum *fibris* ipsorum *transversis* et *horizontalibus*, facillime in iisdem dignoscuntur, et *incrementa* ipsorum *annua* clarissime in iisdem apparent; ut alia ejusmodi plura taceamus. Quando frustra etiam illa lignea in justæ magnitudinis acervos convectuntur, hique eadem illa enchiresi tractantur, qua carbonarii nostri in parandis ex ligno recenti carbonibus utuntur, optimæ notæ *carbones lignei* inde prodeunt; qui ab acido vero, quod magna copia in iisdem adhuc residet, minerali non eosdem ubivis cum aliis carbonibus ligneis usus præstant. Ligna enim hæcce fossila, quorum formam situmque hætenus descripsi, tanta *bituminis aluminisque* copia imprægnata sunt, ut non ignem solum facile concipiant, alantque, sed et cineres, qui ab illorum sub diu facta combustionem supersunt, magnam aluminis copiam fundant: quod ex iisdem etiam in peculiari huic usui destinata officina, magna copia, in eodem montis vertice adhuc excoquitur. Atque hæc causa etiam

esse videtur, cur elegantissimi inter ligna ista non raro reperiantur *pyritæ*, qui aliorum similibus more scintillas ex chalybe copiosissimas eliciant; aeri vero liberi per aliquod tempus expositi, primum dehiscere, mox in pulverem resolvi, toti quanti soleant, qui aluminis sulphurisque vulgaris floribus quasi, paucaque terra martiali, vel nudo oculo constare observatur. Quoniam inter eosdem vero nonnunquam reperiuntur, qui arborum fruticumque ramentis circumfusi formas elegantissimas referunt, quos ab interitu suo, si fieri posset, conservare cupiebam, aquam ferventem aliquoties singulis diebus ipsis tamdiu affudi, donec odore et colore fere omni carens ab iisdem rediret: atque hac equidem ratione factum est, ut illorum nonnulli, probe prius exsiccati, atque chartis thecisque siccis involuti, et ab aeris humidioris accessu defensi, jam sex fere annos perdurent, parumque mutationis adhuc passi sint. Horum nonnulli media frusti cujusdam lignei substantia ita sæpe occultantur, ut non nisi illo fracto, fissoque, in conspectum predeant.

Alter horum montium, de quibus dicere constitutum est, in ipsa Hassia, prope Allendorffium, quod a fontibus et officinis suis salaribus longe lateque his in regionibus celebratur, situs, atque ab accolis propterea procul dubio *der Wyszner*, mons albus, appellatus est; quod cacumen ipsius diu adhuc ab incumbente nive albescit, quando in reliquorum montium circumjacentium verticibus dudum illa disparuit. Hic mons priorem istum altitudine multum adhuc vincit, atque ex observationibus barometricis, in eodem etiam aliquoties repetitis, supra Werræ, ipsum quoque proxime præterlabentis, ripam ad 1970 elevatur.

citer pedum Parisinorum altitudinem quam proxime ascendit. In montis hujus viscera, circa 1500 circiter pedum inde a basi ipsius altitudinem, ab orientali, meridionali, et occidentali ejusdem plaga *cuniculi*, quos *mellen* Germani nostri vocant, per durissimum partim lapidem ducti sunt, quos magna bituminis fossilis lignorumque fossilium copia undique ambit. Horum unum, substructionibus suis munitum, sumto metallifossoris habitu, ad *quingentarum* circiter *orgyarum*, quarum singulæ hexapedam Gallicam paulum adhuc superant, longitudinem, finemque adeo ipsum illius cuniculi, subii, nihil ab omni latere, tectoque incumbente, quam *bituminum lignorumque fossilium* immensam copiam, post substructiones illas ligneas, ad lampadum flammam cum commitibus meis videns. Atque bitumen quidem, quod hic reperitur, et eruitur, nigredine et splendore suo ad gagatem ipsum quam proxime accedit, duritie vero parum per ab eodem differt. Lignum vero fossile ejusdem cum altero illo, quod modo descripsi, et a cujus antea indicata sede hic mons etiam paucis miliaribus Germanicis solum distat, videtur esse generis; aluminis vero, aut alius salis mineralis, bituminisque, quantum adhuc constat, parum admodum continet, coloris etiam minus fusci, et nigricantis est. Per hoc lignum cuniculus ille, quem subii, ita ductus erat, ut hunc ipsum undique ambiret, cuniculique ipsius substructionibus in situ suo naturali conservaretur, ejusque altitudinem ab utroque cuniculi latere 4 aut 5 *orgyarum* plerumque esse fossiores confirmabant. Hisce lignis *bituminis* illius, quod modo indicavi, stratum, ad *duarum* circiter *orgyarum* altitudinem, adeo exacte ubivis incumbit, ut superfusum quasi eidem esse videatur :

deatur: unde et proxime eidem subjacentium superiorum lignorum frustra multum eodem imprægnata sunt, propiusve ad colorem ipsius nigricantem accedunt. Quum hujus igitur bituminis tanta intra montis hujus viscera reperiatur copia, ut a ducentis fere annis jam ad excoquenda salia Allendorffiensia sit adhibitum, illoque ipso, quum ibidem adesset, tempore vel singulis annis ultra ducenties mille modios, mensuræ Goettingensis, ex eodem adhuc erui, earundemque salinarum usibus impendi, dicerentur; ligna ista fossilia, quæ eodem bitumine imbuta non sunt, intacta fere a fossibus relinquuntur, futurorum sæculorum usibus demum inservitura. Quemadmodum hæc vero bitumina lignis subjacentibus fossilibus, ita bituminis hujus superficiei *tectum*, quod ipsum proxime excipit, *lapideum* adeo arcte ubivis incumbit et cohæret, ac si vel ipsum infusum eidem esset, adeo ut lapidum bituminumque substantiæ sibi invicem quasi permixtæ ibidem multis in locis appareant. Quanta tecti hujus incumbentis lapidei crassities sit, non satis equidem constat, neque in omnibus forsan locis est eadem. A tecti vero hujus cum subjacentibus bituminibus confinio ad summum montis, ad quod per acclivitatem ejus passim ascenditur, cacumen, *quingentorum* circiter *pedum* Parisinorum altitudo adhuc multis in locis est: hocque montis cacumen procerarum arborum sylvis passim tegitur, superius vero, ubi in amplissimam tandem planitiem obit, prato quodam satis spatioso et udoso simul infestnitur. Ligna ergo hæc, cum incumbente bituminum strato satis crasso, *mediis* fere montis ingentis *visceribus* inclusa sunt; quum priorum istorum magna pars, non admodum profunde, plerisque in locis, humo cooperta

cooperta sit, parsque solum illorum aliqua sub accumbentis supremi montis verticem, quantum adhuc constat, excurrat. Utrumque tamen tantum naturæ est miraculum, ut fatis vivis depingi coloribus vix queat, neque parum ad illustrandam globi nostri terraquei historiam conferre vel maxime aliquando possit.

Ejusdem forsan generis est, quod ex *Societatis Reg. Goettingensis Commentariorum tom. II.* paucis hic repetere liceat. Reperta nempe ante aliquot annos, et ex marga eruta, in montium nostrorum Hercynicorum vicinia, sunt ossa quædam ingentis, et fere monstrosæ, magnitudinis, quæ Elephantis cujusdam reliquæ primo intuitu esse fere omnibus videbantur. Paulo accuratiore vero illorum instituto examine, facile apparebat, ossa ista animalis cujusdam fragmenta esse, minoris quidem elephante altitudinis, ejusdem tamen, si non majoris, cum eodem molis et ponderis. Quum eodem vero in loco paulo post etiam *dentes* eruerentur, inusitatæ magis formæ, quam magnitudinis, qui vivi istius, per nonnullas Germaniæ Galliæque provincias tunc temporis peregrinatis, rhinocerotis dentibus exacte similes reperiebantur esse; *rhinocerotis* ossa ista fossilia esse, eo minus relinquebatur dubii, quod nullius adhuc cogniti, sive terrestris, sive aquatilis, animalis similitudinem ista ullam referrent. Tot vero eorum ossium eodem in loco reperta sunt fragmenta, ex quibus clarissime patet, *quatuor* minimum *adultæ*, unumque *minus adultum*, ejusdem generis individua eodem in loco sepulta fuisse: id quod occasionem mihi postea dedit, viam quandam novam tentandi, qua corporum hujusmodi peregrinorum, ipsorumque marinorum, in terræ continentis diversis regionibus origo intelligi forsan melius posset: de quo
in

in iisdem Societatis nostræ *Commentariorum tom. IV.*
jam fufius dictum est.

*L. Experiments in Electricity: In a Letter
from Father Beccaria, Professor of Expe-
rimental Philosophy at Turin, to Benjamin
Franklin, L. L. D. F. R. S.*

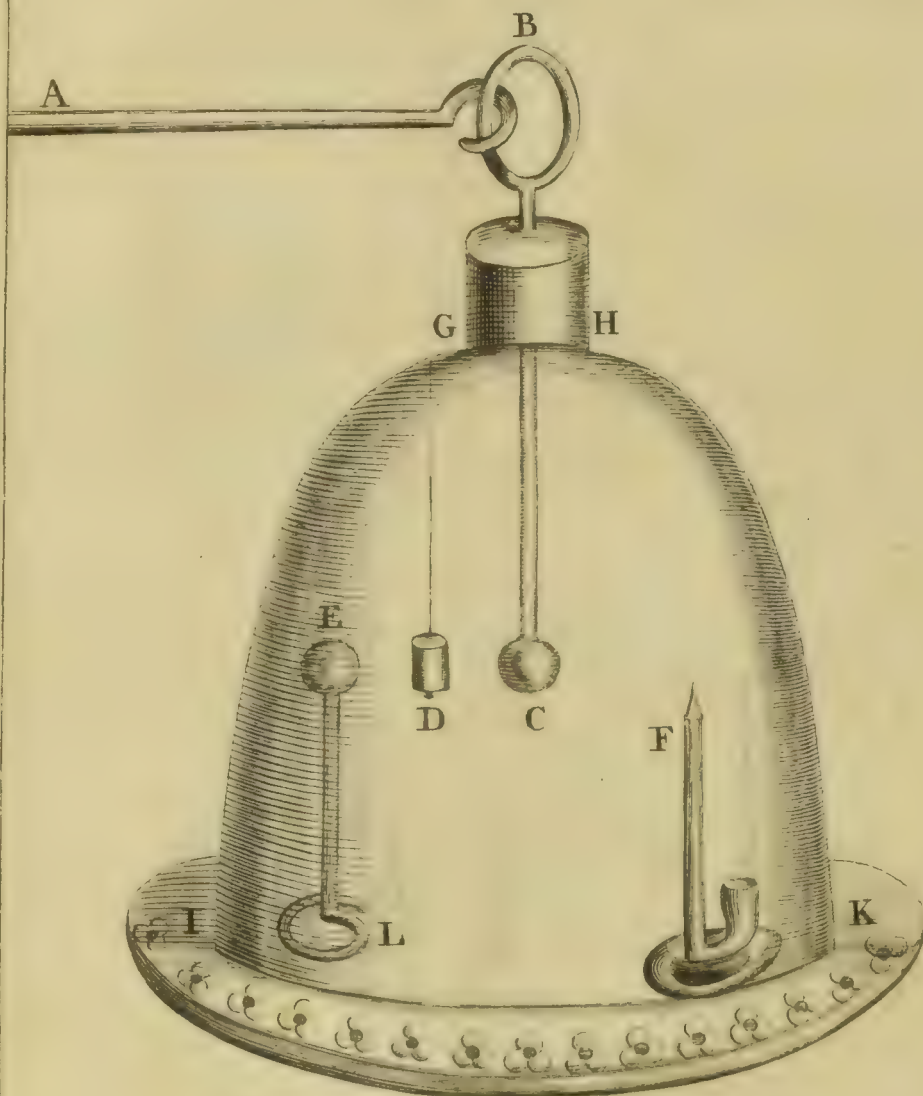
*Benamino Franklin viro de re electrica meritissimo
Joannes Baptista Beccaria ex scholis piis S.P.D.*

Read Feb. 14, 1760. I. **S**ospitem ex America Londinum te

appulisse gaudeo, vir præclaris-
sime. Offero tibi de motibus electricis, qualem experi-
mentis excudi, hypothesin: partior hanc in duas
partes, quemadmodum ipsa postulare videtur motuum
hujusmodi differentia: ago parte prima de accessioni-
bus, de discessionibus dico parte altera.

2. Et continuo universam de accessionibus pertrac-
tationem meam ita paucis comprehendo: “ Quum
“ ignis electricus copiosior in corpore altero vi ex-
“ pandendi se ad æqualitatem trajicit in alterum,
“ partem interjecti aeris dimovet e loco pro majore
“ sua copia majorem: fit inde, ut aer a tergo cor-
“ poribus adjacens deficiente fulcro aeris intermedii
“ ruat versus ipsum medium locum; ruit vero non
“ circumeundo corpora, quæ via est longior, atque
“ impedita abs igne profluente, sed ipsa trudendo a
“ tergo: hac aeris trusione accedunt corpora inæqua-
“ liter electrica; dum accedunt, accelerantur, quia
“ pro accessione majore copiosior ignis interfluit, co-
“ piosior





“ piosior intermedius aer disjicitur, augeturque adeo
 “ momentum aeris a tergo trudentis.”

EXPERIMENTUM I.

3. Aerem disjici ab interfluente electrico igne, scintillæ experimento demonstratur, quæ excitatur in medio tubulo, hinc clauso hermetice, inde impedito abs liquore aliquo, ipsa enim, disjiciens liquorem, disjectum aerum testatur.

4. Neque vero interfluens ignis aeris intermediï disjecti fulcrum potest supplere, quo aer corporibus a tergo adjacens sustineatur; is enim liberrime effluit, et effunditur per alterum corpus: quare cum non fulciatur ipse, nec fulcri vices potest agere.

EXPERIMENTUM II.

5. Experimento res confirmatur plane luculentissimo: vittam ex charta inaurata longam pollices octo, latam lineas quatuor convolvo circa sese, ut cylindrum solidum D [*Vide Tab. XII.*] efformet: hunc suspendo serico filo DG sub fornice vitri pneumatici IHK, per cujus verticem H init virga metallica BC definens in metallicam sphæram C: figo in lance IK virgam metallicam LE simili sphæra ornatam; nempe sphæræ C et E sunt in eodem plano cum pendulo D, atque ab ipso distant ambæ æqualiter; tum nexa catena AB, quæ pertinet ad machinam electricam virgæ BC, et consequenter immisso electrico igne in virgam BC, observo: 1. ante subductum aerem chartaceum cylindrum D agitari vehementissimis vibrationibus inter duas sphæras EC: 2. dum aer subducitur, minui eam agitationem plane pro copia subducti aeris: 3. omni, quantus potest, aere subducto, cylindrum D vix

nutare. His observatis aerem restituo particulatim; atque pro aere admissō video vibrationes iterum augescere, et fieri, quemadmodum antea, vehementissimas: quæ dum contemplor singula, atque omnia simul comprehendo animo, intelligo magnitudinem motuum electricorum aeri, vel toti, vel residuo proportionē respondere.

EXPERIMENTUM III.

6. Quod leviora corpuscula, bracteolæ, et pulvisculi, motu adhuc satis sensili agitentur in rarefacto aere, id in causa fuit, cur censuerim a principio cum physicis aliis electricos motus actioni ignis electrici in aerem acceptos referri non debere; quod, re nunc accuratius collata, residuos videam in residuo aere motus non majorem habere ad primos motus, qui fiunt in toto aere atmospherico, proportionem, quam ea sit, quæ inter residuum et primum aerem intercedit, id nunc facit, ut illum meum, et alienum errorem agnoscam, atque in contrariam veniam, quam verissima testatur experientia, sententiam.

7. Neque omittam partem observationis opportunissimam rei confirmandæ: soleo enim allatum ultimo loco experimentum persæpe capere in cubiculo obtenebrato, atque tum observo ignem vibrantem pendulum ante aerem subductum micare hinc inde crepitantibus quidem, ac lucidissimis, sed arctis, atque brevibus prope sphæram utramque scintillis: deinceps ignem hunc pro subducto aere, adeoque pro minutis penduli vibrationibus effluere magis amplo radio, magis tranquillo, magis continuo, quanquam non ita micante.

8. Atque

8. Atque hæc ipsa res certe reactionem aeris demonstrat ignem electricum cohibentis in densitate quadam, quæ densitati suæ proportioni respondeat; hæc autem aeris reactio non potest actioni non respondere ignis electrici aerem nitentis disjicere, ac plane disjicientis, ubi necessaria is igni vi polleat.

9. Atque his plura non adferam tibi pro accessionibus explicandis corporum electricorum inæqualiter, attuli in datis ad Beccarium litteris, in quibus tamen non nisi dubitanter plura admodum differebam de mechanica accessionum causa: accuratior consideratio experimentorum facit nunc, ut de accessionum causa dicam fidentius.

10. Corpora æqualiter electrica discedunt; qua causa id efficiatur, nunc persequar.

11. "Ignis electricus, qui quoquomodo insidet in corporibus, nititur, ut libretur cum igne electrico, qui insidet in aere. Si uterque" [finito me illum deinceps proprium dicere hunc aereum] "æqualem habet proportionem ad nativam corporum atque aeris capacitatem, corpora accedunt, aut in nativa quiescunt directione; alter si superat proprius, vel aereus, corpora explicat similiter, compellitque ad discessionem." Hæc rei narratio.

EXPERIMENTUM IV.

12. Quoties in cubiculo quodam meo amplo ipso quidem at clauso pergo diutius urgere alium, atque alium in catenam electricum ignem, exterior partem ejus ignis effundi in aerem ambientem, ipsumque etiam, quantuscumque in cubiculo est [dum a tempestate siccus sit, vel abs igne] facere excessu electricum. Duo enim tenuia ex lino fila, quæ vel summis

digitis prehensa, vel arundine suspensa deffero quolibet, in quodlibet cubiculi spatium, mutuo discedunt. Atque, sive in eodem ipsa contineam loco, sive in loco transferam alia atque alia, pergunt divergere ad semihoram, ad horam quandoque, et ultra.

EXPERIMENTUM V.

13. Si ea fila ope vitri separentur ab arundine, adeoque a solo, principio divergunt, tum ad nativam veniunt directionem: obtingunt autem hæc omnia post omnes elicitas, quantæ possunt, a catena scintillas, post omnem ab ipsa subductum redundantem ignem, post catenam nexam cum solo.

EXPERIMENTUM VI.

14. Ex diuturna electricitate machinæ phænomena experior omnino eadem. At, si diuturna electricitas machinæ continuo consequitur diuturnam electricitatem catenæ, aut contra, fila, quæ arundine suspensa constanter divergere videbam ex alterius electricitate, video principio accedere mutuo incipiente electricitate alterius, tum iterum divergere quemadmodum antea.

15. His inquam experimentis doceri nos arbitror.
 “ 1. Dum alium atque alium ignem congerimus in
 “ catenam, aliam atque aliam ipsius partem disce-
 “ dere quam latissime in ambientem aerem, ipsique
 “ adhærescere, atque ita omnem contentum in cubi-
 “ culo aerem evadere excessu electricum: similiter
 “ dum alium atque alium ignem subtrahimus a ma-
 “ china, aliam atque aliam ignis partem machinam
 “ haurire abs aere ambiente; atque ita omnem con-
 “ tentum in cubiculo aerem fieri defectu electricum.
 “ 2. Fila vel manu, vel arundine delata discedere
 “ in.

“ in aere, vel electrico excessu ex catena, vel electrico
 “ defectu ex machina, vel vi proprii nativi ignis su-
 “ perantis aereum, si merguntur in aere electrico ex
 “ machina, vel vi ignis aerei superantis nativum pro-
 “ prium filorum ignem, si merguntur in aere electrico
 “ ex catena. 3. Fila hujusmodi divergentia ex elec-
 “ tricitate catenæ, ex consequente electricitate ma-
 “ chinæ (vel contra) primo accedere, quia machina
 “ primo hauriat abs aere excessum inditum a catena,
 “ vel catena ignem restituat aeri sublatum a machina;
 “ atque ita ignis in aere ad nativam mensuram re-
 “ ductus librari debeat cum igne nativo permanente
 “ in filis. 4. At deinceps ex diuturniore electricitate
 “ machinæ consequente electricitatem catenæ, vel
 “ contra, fila iterum divergere, quia vel machina per-
 “ gat haurire partem ignis nativi, vel catena pergat
 “ addere alium ultra nativum aeri ignem. 5. De-
 “ nique fila eadem cum vitro separantur, principio
 “ divergere, quia ipsa, quæ solum ignem nativum
 “ habeant, mergantur in aerem vel excessu, vel de-
 “ defectu electricum. At dein, cum sunt vitro sepa-
 “ rata, venire ad nativam directionem, quia vel pars
 “ ignis aerei, si aer est excessu electricus a catena, in
 “ ipsa se denique effundat ad æqualitatem, quin ultro
 “ præterfluat ob cohibens vitrum; vel pars ignis na-
 “ tivi et proprii filorum se effundat in aerem, si aer
 “ est electricus defectu, quia suppleri aliunde possit
 “ ob interdicens vitrum.”

16. Quare hæc videntur universe constare :
 “ 1. Aerem suum continere nativum ipsi sibi ignem.
 “ 2. Nativo aeris igni ignem alium addi posse, na-
 “ tivi abs aere ignis partem posse subtrahi, sed lente,
 “ et quasi per vim. 3. Aerem dimittere redundan-
 “ tem,

“ tem, haurire ignem deficientem, sed quam lentif-
 “ sine. 4. Si aer æque ac contenta corpora habeant
 “ nativum ignem suum, consistere hæc in nativo
 “ statu; si aer, manente nativo in corporibus igne,
 “ ignem habeat nativo aut copiosiore, aut parcio-
 “ rem, corpora discedere.” Verum singula hæc theo-
 remata experimentis aliis illustrari mirum in modum
 possunt, et demonstrari latius.

EXPERIMENTUM VII.

17. Duo fila nexa catenæ post paucas globi conver-
 siones divergunt maxime; deinceps a diuturniore ca-
 tenæ electricitate incipiunt minus divergere; atque
 hæc divergentia ad certum usque gradum minuitur,
 ut quandoque angulus divergentiæ post continuatam
 diutius globi frictionem fiat subduplus anguli diver-
 gentiæ, qui extiterat ab electricitate eadem primo ex-
 citata. Quum hanc filorum divergentiam video mi-
 nutam satis, tum globum non frico ulterius, sed sti-
 lum acutissimum catenæ admoveo pedetentim, quo
 lente redundantem omnem abs ipsa ignem hauriam.
 Id dum fit, lente fila accedunt, tum continenter
 lente iterum divergunt ad angulum primo quidem
 minorem, sed satis magnum, quæ quandoque vix
 metiantur gradus quadraginta quinque; tum vero
 pergunt divergere ad diuturnum tempus.

18. Videlicet maximus abs electricitate primo ex-
 citata angulus, quia tum satis ingens immissus in ca-
 tenam, et in nexa fila, ignis; nullus, vel minimus, in
 aerem. Fit deinceps minor angulus, prout augescit
 ignis, qui consequenter immittitur in aerem, con-
 stante eodem ignis excessu in catena. Dum admoto
 stilo lente minuitur ignis in catenam immissus, mi-
 nuitur

nuitur adhuc angulus prout accedunt ad æqualitatem ignis in catena residuus, atque ignis in aerem immisus, ipsique affixus; ubi uterque ignis pertingit ad æqualitatem, fila attingunt ad directionem suam nativam; cum deinceps minuitur ulterius ignis redundans adhuc in catena, nec similiter minuitur ignis redundans in aere [aeri enim, uti vidimus, adhærescit, atque ab ipso lente dimittitur] fila iterum discedunt pro excessu ignis aerei supra ignem in catena; qui denique aereus ignis, quoniam lentissime dimittitur, diutissime pergunt divergere fila.

EXPERIMENTUM VIII.

19. Si fila sint nexa machinæ, eadem omnia in iis contingunt phænomena ex diuturna electricitate machinæ, quæ in iis contingebant catenæ nexis ex diuturna electricitate catenæ.

20. Quare universe, quemadmodum constitui a principio, “ Si ignis proprius corporum, quantum-
“ cumque is sit, libratur cum igne ambientis aeris,
“ corpora vel consistunt in nativo statu, vel ad ipsum
“ redeunt; si alter ignis superat alterum, corpora dis-
“ cedunt vi ignis superantis.”

21. Atque hinc rationem vides, ni fallor, vir præstantissime, cur æque discedant tum quæ a catena, tum quæ a machina sunt electrica corpora. “ Ignis
“ proprius in corporibus a catena electricis vincit
“ aereum, aereus vincit æque proprium in electricis
“ a machina.”

EXPERIMENTUM IX.

22. Quoties fila mutuo discedunt a se invicem, atque divergunt ex aucto in ipsis, vel minuto igne
nativo

nativo ad meum accedunt admotum digitum: quoties discedunt, atque divergunt servantes ignem suum nativum ex aucto, vel minuto igne nativo aeris, a digito refugiunt in eo.

23. Hac vero res primo quidem notissimis legibus accessionum atque discessionum electricarum est consentanea; quum enim natus in filis ignis auctus est, vel minutus, ipsa sunt inæqualiter ac digitus electrica, qui habet nativum ignem suum: quum vero natus ignis perstat in filis, et ipsa discedunt mutuo ob ignem auctum, vel minutum in aere; tum, quemadmodum discedunt ambo alterum abs altero, ita discedant oportet a digito meo ipso etiam habente ignem nativum, vi aeris, in quo natus ignis minutus est, aut auctus. Sed præterea hæc filorum a digito discessio ipsa hanc probat etiam, quam affero, discessionis causam: nam et digitus, et fila cum solo communicant, adeoque alium in se certe non habent ignem, nisi nativum, quo quoniam non discedunt, restat ipsa discedere ex nativo igne in aere aucto, vel minuto.

24. Cum in hanc primo incidi discessionum electricarum theoriam, ea me ancipitem tenuit suspicio, ne ignis aereus discessionem facere non posset in machina iis æquales in pernecitate, et magnitudine, quas facit in catena ignis proprius: hæc inde subibat animum suspicio, quod ignis in machina, et catena (universæ in corporibus omnibus deferentibus) mensura mutetur quam pernecissime, lentissime autem augeatur, aut minuatur mensura ignis in aere ambiente. Alia præterea suspicionem exaggerabat consideratio; tantum me nunquam abs aere, quantum a machina, subtrahere ignem potuisse, tantum potuisse nunquam aeri addere, quantum catenæ; nunquam enim eo veni,
ut

ut omnem aerea sola electricitate sustulerim filorum divergentiam, quanta existerat ex integra machinae aut catenae electricitate.

Factum inde est, ut experimenta hæc eadem, imo et alia, in literis attulerim ad præclarissimum instituti Bononiensis præsidem datis, quin hanc in iis fatis late theoriam discessionum constituerem.

Verum re maturius perpensa intellexi pernecitatem discessionum, quæ fiant abs igne aereo, non ipsi pernecitati respondere debere, qua vel augeatur ipsius copia, vel minuatur; sed pernecitati, qua agat copia inhærens; et magnitudinem discessionum, quæ fiant ab igne eodem, non absolutæ, sed comparativæ ipsius magnitudini respondere debere; utrumque autem facile vidi pro re obvenire.

EXPERIMENTUM X.

25. Si in experimento VI. post minutam ex diuturniore electricitate catenae filorum divergentiam, non lente, sed protinus (manu nempe in catenam immissa) omnem subtraho a catena redundantem ignem, continuo incipiunt quidem accedere fila quam velocissime; verum velocitate æquali resiliunt repente, antequam pertingant ad nativam directionem. Hæc autem altera discessio quoniam fit ex aereo igne superante proprium in filis residuum, video inde ignem aereum (quamquam mutetur lentissime ipsius copia) ipsum tamen discessionem facere iis æque veloces, quæ abs igne proprio efficiuntur.

EXPERIMENTUM XI.

26. Sed et ipsas ex igne aereo obvenire æque magnas ita intelligo. Post ignem inditum in aerem, qui

catenam ambit quantus potest, dempto redundante omni ab catena igne, atque adeo discedentibus jam filis ex igne aereo, vitreo globo substituo sulphureum, atque ex hujus functione, video, principio augeri angulum divergentiæ, quem effecerat ignis aereus, tum ipsum continenter minui.

27. Id autem certum: primas globi sulphurei frictiones demere partem nativi ignis a catena, neque pariter demere excessivum ignem ambientis aeris (aer enim lente, ut vidimus, accipit ignem alienum, lente acceptum exuit); quare augefcit magnitudo divergentiæ, quæ fit abs igne aereo, non mutata magnitudine absoluta ignis aerei, sed mutata solum magnitudine ipsius comparativa, minuto nempe igne proprio; adeoque aucta proportionè aerei ad proprium. Igitur quoniam quantum ignis ingeritur in catenam, tantundem subtrahitur a machina; proportio ignis aerei circa machinam ad residuum in machina eadem erit ac proportio ignis in catena redundantis ad aereum circa catenam: quamobrem aereus circa machinam discessionem faciet non æque veloces solum, sed et æque magnas, ac eæ sunt, quas facit ignis proprius in catena.

28. Itaque, ut omnia demum paucis complectar, quæ pertinere videntur ad motus electricos universe omnes explicandos (suspensiones enim, adhæfiones, vibrationes, infinitosque alios compositiores quasi ludos electricos, tu ipse probe videre visus es discessionibus omnes, atque accessionibus contineri) hæc denique est summa hypothesis meæ. “ Accessiones corporum inæqualiter electricorum efficiuntur ab igne
“ electrico a corpore altero, in quo copiosior est, effluente in alterum per aerem interjectum, ipsum-
“ que

“ que disjiciente. Disceffiones autem vel igne pro-
 “ prio corporum efficiuntur se expandente contra
 “ aereum, vel aereo se expandente valentius contra
 “ proprium;” quæ tamen expansio ignis alterius al-
 terum superantis sine mixtione mutua alterius cum
 altero videtur contingere. Aer enim ignem continet
 suum quantuncumque, ne discedat; arcet proprium
 deferentium corporum ignem, ne adhærescat sibi.

29. Hanc, inquam, hypothefim offero tibi, vir præ-
 clariffime, quo tu eam facias meliorem. Si tanti in-
 terea ipfam ducas, ut Regiæ ifti Scientiarum Societati
 exhibenda videatur, res ex meo obtinget defiderio,
 qui quando ornamentum ipfi adferre nullum possum,
 diligentiam faltem nolim defiderari meam. Tu vero
 cura, ut valeas. Servari enim te decet quam diutif-
 fime utiliffime, Scientiæ perficiendæ amplificandæ-
 que, quam certiffimam instituifti. Dat. Taurini,
 24 Decembris, 1757.

*Remarks on the preceding Paper, by Benja-
 min Franklin, L. L. D. F. R. S.*

Read Feb. 14, 1760. **F**OR the better understanding this pa-
 per, it is neceffary to know, that
 Father Beccaria ufes a large chain, fufpended by filk
 lines, for the purpofe of a prime conductor; and that
 his machine for turning the glafs globe is fo con-
 trived, as that he can, on occafion, readily ifolate it,
 (i. e. place it on glafs or wax) together with the per-
 fon that works it. When the communication is thus
 cut off between the earth and the chain, and alfo
 between the earth and the machine, he obferves,
 that the globe being turned, both the chain and the

machine show signs of electricity; and as these signs, when examined, appear to be different in the chain, and in the machine, and the globe having, as he supposes, drawn from the machine part of its natural or common quantity of electricity, and given it to the chain, he calls the electricity appearing in the chain, electricity *by excess*; and the electricity appearing in the machine, electricity *by defect*; which answer to our terms of *positive* and *negative* electricity, or electricity *plus* and *minus*. And thus his expressions, *electrifying by the chain*, and *electrifying by the machine*, are to be understood, electrifying *positively*, and electrifying *negatively*.

LI. *An uncommon Case of an Hæmoptysis ;
by Erasmus Darwin, M. D.*

*To the very Honourable and Learned the President,
and Members of the Royal Society.*

Gentlemen,

Read Feb. 14, 1760. **T**HE following case of a discharge of blood from the pulmonary artery, appears to have been owing to a cause different from any mentioned amongst the writers of medicine: and as, from the knowledge of that cause, the cure was so easily deduced, I flatter myself, you will not esteem it unworthy your attention.

A gentleman, residing near this place, between forty and fifty years of age, of a pale and meagre habit, has been daily afflicted with violent head-achs
for

for several years past; and, about four years ago, after having taken a considerable quantity of Peruvian bark, became suddenly paralytic. The use, however, of his right limbs was so much restored, as only to remain weaker than the other; when, upon suddenly awaking from his sleep about two o'clock in the morning, (May 7, 1759), he spit up four or five ounces of florid blood.

He immediately lost twelve or fourteen ounces from the arm, had elixir of vitriol given him, and in the evening had a glyster, and lost blood again to about ten ounces.

On the 8th, about the same hour, he again suddenly awaked, and spit about the same quantity of blood as before. He was now advised to increase the quantity of elixir of vitriol, had a bolus of *extractum Campechense* every six hours, and had a leech applied to a blind pile, that had long appeared after going to stool.

On the 9th, at the same hour, he had again the same discharge as before. That these hæmorrhages were from the pulmonary artery, rather than the bronchial, appears from the sudden exspuition, the quantity, the floridity, and from the discharge being without pain, and unmixed with phlegm.

As he had no feverish symptoms, either when he first awaked, or during the day, no more blood was taken from him; and as he constantly slept profoundly from ten o'clock till two, when the complaint seized him, he was now advised to be awakened, and rise out of his bed, at one in the morning, and remain awake till three, omitting all medicines.

He

He continued to rise from bed for a week, and has ever since used himself to awake at the same time; and has not only been entirely free from this complaint, and that without any further discharge from the hæmorrhoidal vessels; but has got more flesh, and his head-achs are become even inconsiderable.

I ought not here to omit, that he had a vomit given him on the 12th, and twice repeated, at the intervals of three or four days.

As the patient, from a former hæmiplagia, had, in all probability, many parts of his body rendered less irritable than is natural; and as he constantly slept profoundly, and the hæmoptoe always awaked him after four hours sleep; I was led to conclude, that, during this sleep, the lungs were not sufficiently sensible to push forwards the whole circulation; and and that hence the blood, gradually accumulated, ruptured some minute branches of the pulmonary artery, before the uneasiness became great enough to awake the patient. And, as much as the evidence of a single case in medicine may be estimated, the successful cure would seem to evince the truth of this doctrine.

I have only to add, that the anxiety, with which patients reduced to great weakness awake from their sleep, and the hurried pulse, have, by others, been observed to be owing to an accumulation of blood in the lungs, during their state of decreased sensibility: And how detrimental, in these cases, might be the administration of opium, or nitre; whilst the want of sleep, or the recurring hæmorrhage, might seem, to the unwary practitioner, to need their assistance.

After a few days, observing some cough remain, it seemed adviseable to give two or three vomits; as, from late experience, they do not endanger a renewal of the discharge, and must promote the expectoration of the eschar, or any extravasated blood; which otherwise, by its delay acquiring a putrid acrimony, perhaps most frequently erodes the contiguous vessels, and, forming new ulcerations, becomes the general cause of consumptions, subsequent to accidental spitings of blood.

I am, Gentlemen,

Your very humble servant,

Lichfield, Dec. 17. 1759.

Erasmus Darwin.

LII. *An Account of the late Earthquakes in Syria: In a Letter from Dr. Patrick Russell, to his Brother, Alexander Russell, M. D. F. R. S.*

Aleppo, Dec. 2, 1759.

Read Feb. 21,
1760.

AS I recollect nothing in the way of business, which I have not already wrote you about; and as I know not when this letter may get away, I shall, in the mean time, give you some account of the earthquakes here, which have thrown the people into a terrible consternation.

The spring of this year was unusually dry, the summer temperate, and the autumn, though the rains came on towards the end of September, might
be

be esteemed much drier than in other years. The Aleppo river has been very low all the summer; and its bed, from the first to the second mill, is, I believe, even now still without water. This phenomenon I, at first, thought remarkable; but have been informed, that the scarcity of water complained of, during all the summer, was occasioned by driving the river into some rice-grounds lately formed towards Antab.

On the morning of the 10th of June, a slight shock of an earthquake was felt here, and, as usual, soon forgot; having, so far as we know, been felt in no other place, in any degree of severity.

October the 30th, about four in the morning, we had a pretty severe shock (indeed the most violent I had ever felt), which lasted somewhat more than a minute, but did no damage in Aleppo. In about ten minutes after this first, there was a second shock; but the tremulous motion was less violent, and did not last above fifteen seconds. It had rained a little in the preceding evening; and when the earthquake happened, the west wind blew fresh, the sky was cloudy, and it lightened.

This earthquake occasioned little alarm amongst the natives, and even with the Europeans was the topic only for a day. But the subject was soon revived, by letters from Damascus, where the same shock felt by us at Aleppo, and several other successive ones, had done considerable damage. From this time, we had daily accounts of earthquakes from Damascus, Tripoly, Seidon, Acri, and all along the coast of Syria; but so exaggerated in some circumstances, and so inaccurate in all, that we only knew
in

in general, that Damascus, Acri, and Seidon, have suffered injury from the earthquake, though less than was at first given out.

Such stories circulating among the people, rendered them more susceptible of alarm; and an occasion soon offered, where they feared, that the worst of their apprehensions were to be realized.

This happened the 25th of November. The morning had been serene, some clouds arose after noon, and the evening was remarkably hazy, with little or no wind. About half an hour after seven at night, the earthquake came on: the motion, at first, was gently tremulous, increasing by degrees, till the vibrations became more distinct, and, at the same time, so strong, as to shake the walls of the houses with considerable violence; they again became more gentle, and thus changed alternately several times during the shock, which lasted in all about two minutes. In about eight minutes after this was over, a slight shock, of a few seconds duration, succeeded. The thermometer was at 50, and the barometer stood at 28.9, the mercury suffering no alteration. There was little or no wind in the night, the sky clear, excepting some heavy clouds, that hung about the moon. At a quarter after four next morning, we had another shock, which lasted somewhat less than a minute, and was hardly so strong as that of the preceding night. The night of the 26th was rainy and cloudy. At nine o'clock we had a slight shock, of a few seconds. The motion here appeared to be very deep, and was rather undulatory than tremulous. The 27th, cloudy and rainy. From midnight of the 25th, besides these now mentioned, four or five slighter

shocks were felt; but I myself was sensible of none; till the morning of the 28th, when we had a short pulsatory shock. The same day, at two o'clock, we had a pretty smart shock, lasting about forty seconds. From this time, I was sensible of no more, though others either felt or imagined several slight vibrations every day.

However violent this earthquake was, or rather, by the frightened people of Aleppo, imagined to be, it is certain, that, excepting in a very few old walls, the city bears no fresh marks of ruin; none of the oldest minorets have suffered. Its effects at Antioch were more formidable; many houses have been thrown down, and some few people killed.

December 7 h.

THE earthquake of the evening of the 25th has proved fatal to Damascus; one-third of the city was thrown down, and of the people, numbers yet unknown perished in the ruins. The greater part of the surviving inhabitants fled to the fields, where they still continued, being hourly alarmed by slighter shocks, which deterred them from re-entering the city, or attempting the relief of such as might yet be saved, by clearing away the rubbish. Such was the purport of a letter I read this day, which was wrote from Damascus three days after the earthquake. Other accounts we have at this place, make the loss of the inhabitants amounts to 20,000; but, in circumstances of such general horror and confusion, little accuracy can be expected, and the eastern disposition to exaggeration reigns, at present, universally.

Tripoly has suffered rather more than Aleppo; three minorets, and two or three houses, were thrown down,

down, while the walls of numbers of the houses were rent. The Franks, and many of the natives, have deserted the city, and remain in the fields.

At Seidon, great part of the Frank kane was overthrown, and some of the Europeans narrowly escaped with their lives. Acri and Latakea have suffered little, besides rents in some of the walls: but Saphet (eight hours from Acri) was totally destroyed, together with the greater part of the inhabitants.

Such are the most authentic accounts we have from abroad; how much they contribute to calm the minds of the people at Aleppo, you may easily imagine. Since the afternoon of the 28th, several slight shocks have been every day felt, and many more formed by the power of imagination: for my own part, I have since that time perceived none, excepting one the evening of the 5th, which was pretty strong, but lasted not above twenty seconds. The weather, for these two days, has been gloomy and rainy; a change which people are willing to flatter themselves is favourable. It often lightens in the night, and thunder is heard at a great distance.

Extract from another Letter of Dr. Patrick Russell, to Dr. Alexander Russell, dated at Aleppo, 29 March 1760.

IN my last, of the 7th of December, I gave you a full account of the earthquakes, which have occasioned an universal panic all over Syria. There were several other shocks in December, and a few very slight ones in January: since which time, all has been quiet.

Z z z 2

+ Except-

Excepting a few old houses, none of the buildings in Aleppo were actually thrown down ; but the walls of a considerable number of houses have been rent.

We have had the wettest winter I ever saw in this country. The Coic has every-where overflowed its banks, and is just now much higher than it has been known to rise for many years.

LIII. *Remarks on the Bovey Coal: In a Letter to the Right Honourable George Earl of Macclesfield, President of the Royal Society. By Jeremiah Milles, D. D. F. R. S.*

My Lord,

Read Feb. 28, 1760. **T**HE description, which the learned professor Hollman has given the Society *, of two remarkable strata of fossil wood in Germany, one in the neighbourhood of Munden, in the duchy of Grubenhagen, and the other near Alendorf in Hesse, corresponds, in so many particulars, with some strata, discovered about fifteen years ago, in Devonshire, that it suggested to me a doubt, whether those German strata were really (what the learned professor supposes them) fossil wood, and formerly a vegetable substance, or (what he says the miners call them) fossil coal. The reasons, on which this doubt is founded, are submitted to the judgment of your Lordship, and the Society, in the following

* See the above, N° xlix. p. 566.

account of the Devonshire fossil, which is commonly known by the name of the Bovey coal. It is found on a common surrounded with hills, called Bovey Heathfield, in the parish of South-Bovey, 13 miles south-west of Exeter, and 3 miles west of Chudleigh. The uppermost of these strata rises within a foot of the surface, under a sharp white sand, intermixed with an ash coloured clay, and underlies to the south about 20 inches in a fathom.

The perpendicular thickness of these strata, including the beds of clay, with which they are intermixed, is about 70 feet. There are about six of each, and they are found to continue eastward, in an uninterrupted course, to the village of Little-Bovey, a mile distant, and probably extend much further. The strata of coal near the surface are from 18 inches to 4 feet thick, and are separated by beds of a brownish clay, nearly of the same dimensions, but diminishing in thickness downwards, in proportion as the strata of coal grew larger; and both are observed to be of a more compact and solid substance in the lower beds. The lowermost stratum of coal is 16 feet thick; it lies on a bed of clay, under which is a sharp green sand, not unlike sea sand, 17 feet thick, and under that, a bed of hard close clay, into which they bored, but found no coal. From the sand arises a spring of clear blue water, which the miners call mundic water, and a moisture of the same kind trickling through the crevices of the coal tinges the outside of it with a blue cast.

Some small and narrow veins of coal are found intermixed with, and shooting through, the beds of clay, forming impressions like reeds and grass, and
very

very similar to those, which are generally found on the top of coal mines. The clay also (at least that part of it, which lies nearest to the coal) seems to partake of its nature, having somewhat of a laminous texture, and being in a small degree inflammable; and amongst this clay, but adhering to the veins of coal, are found lumps of a bright yellow loam, extremely light, and so saturated with petroleum, that they burn like sealing wax, emitting a very agreeable and aromatic scent.

Though the substance and quality of this coal, in its several strata, are much alike, and it is all indiscriminately used for the same purposes; yet there is some difference in the colour, form, and texture, of the several veins. The exterior parts, which lie nearest to the clay, have a greater mixture of earth, and are generally of a dark brown, or chocolate colour; some of them appear like a mass of coal and earth mixed; others have a laminous texture, but the laminæ run in such oblique, waving, and undulating forms, that they bear a strong resemblance to the roots of trees, of which kind I have seen some specimens from Lough Neagh in Ireland, which seem to be the same sort of fossil.

There are other veins of this coal, which lie more in the center of the strata, and abound most in the lowest and thickest bed, the substance of which is more compact and solid: these are as black, and almost as heavy, as pit coal; they do not so easily divide into laminæ, and seem to be more strongly impregnated with bitumen: these are distinguished by the name of stone coals, and the fire of them is more strong and lasting than that of other veins.

But

But the most remarkable and curious vein in these strata is that, which they call the wood coal, or board coal, from the resemblance which the pieces have to the grain of deal boards. It is sometimes of a chocolate colour, and sometimes of a shining black. The former sort seems to be less impregnated with bitumen, is not so solid and heavy as the latter, and has more the appearance of wood. It lies in strait and even veins, and is frequently dug in pieces of three or four feet long, and, with proper care, might be taken out of a much greater length. Other pieces of the same kind are found lying upon them, in all directions, but without the least intermixture of earth, or any other interstices, except some small crevices, by which the pieces are divided from each other, in all directions. When it is first dug, and moist, the thin pieces of it will bend like horn, but when dry, it loses its elasticity, and becomes short and crisp. At all times, it is easily to be separated into very thin laminæ, or splinters, especially if it lie any time exposed to the heat of the sun, which, like the fire, makes it crackle, separate, and fall to pieces. The texture of this fossil consists of a number of laminæ, or very thin plates, lying upon each other horizontally, in which small protuberancies sometimes appear, like the knots of trees; but, upon examination, they are only mineral nuclei, which occasion this interruption in the course of the laminæ; and pieces of spar have been sometimes found in the middle of this wood coal.

Though the texture of this coal is laminated, yet it does not appear to have any of those fibrous intersections, which are observed in the grain of all wood.

This

This coal easily breaks transversely, and the separated parts, instead of being rugged and uneven, are generally smooth and shining, in which even the course of the laminæ is hardly discernable.

They dig this coal in an open pit, together with the clay that is mixed with it; and though it lies very close and compact in its original bed, yet it is so easily separated, that they can afford to sell it for half a crown a ton at the pit. The smaller coal is separated from the clay by a screen, or grated shovel; the larger, which rises sometimes in pieces of above an hundred weight, is piled up by hand. There is hardly any other use made of it at present, but to bake the earthen ware of a manufacture erected at South-Bovey, and for burning of limestone, which rising in great quantities at the neighbouring town of Chudleigh, the coal is carried thither, and they return with limestone to the pit; which they burn there, for the use of the northern parishes, to whom it lies more convenient than the kilns of Chudleigh.

The fire made by this coal is more or less strong and lasting, according to its different veins: those which lie nearest to the clay, having a greater mixture of earth, burn heavily, leaving a large quantity of brownish ashes; that, which they call the wood coal, is said to make as strong a fire as oaken billets, especially if it be set on edge, so that the fire, as it ascends, may insinuate itself between, and separate the laminæ. But that of the stone coal is accounted most strong and durable, being apparently more solid and heavy, and probably also more strongly impregnated with bitumen. One of the proprietors of this coal made an experiment of burning it in the fire-engines of Cornwall,

wall, to which it might have been transported, without much trouble or expence; being only four miles distant from the navigable river Teign; but the heat of the coal was not sufficiently intense, and the consumption of it too great, to answer the purpose. When this coal is put into the fire, it crackles, and separates into laminæ, as the cannel coal does into irregular pieces, burns for some time with a heavy flame, becomes red-hot, and gradually consumes to light white ashes. Though the transverse crevices made in it by the fire give it the external appearance of a wooden brand, yet, if quenched when red-hot, the unconsumed part does not look like charcoal, but seems to be almost as smooth and solid, as when first put into the fire.

The thick heavy smoak, which arises from this coal when burnt, is very fœtid and disagreeable; entirely different from that aromatic scent of the bituminous loam, which is found adhering to it, but much resembling that of the asphaltum, or bitumen of the Dead Sea. The whole neighbourhood is infected with the stench, which is waisted by the wind to the distance of three or four miles. When burnt in a chimney (as it is sometimes in the neighbourhood), the offensiveness is lessened by the draught: however, it is found, by those, who live continually in the smoak of it, not to be unwholesome; nor is it in the least prejudicial to the eyes, like the smoak of wood. The most shining and solid pieces of this coal have not the least degree of electrical attraction.

Notwithstanding the resemblance, which this fossil bears to wood, especially when viewed in detached pieces, yet the following observations on its situation,

its form and properties, will prove it to be not of a vegetable, but of a mineral origin.

In the first place, there does not seem to be any imaginable cause in nature, which could bring together such a mass of fossil wood, as is found in this, and other strata of the like kind in different parts of Europe. It extends here to the depth of 70 feet: in that near Munden they have sunk 50 feet, without coming to the bottom. Fossil trees, though frequently found single, or in small numbers, are generally discovered in morasses and soft ground, where they have either buried themselves by their own weight, or been overwhelmed by some accidental cause: but the Bovey strata are found in a dry soil, intermixed with clay and sand, and, by their regular course and continuance, carry the most undoubted marks of never having been disturbed since their original formation. Fossil trees likewise preserve their form and size, their length and roundness, their branches and roots, their fibrous texture and strength, and are either found entire, or in such large pieces, that there is no room to doubt of their nature, since the very species of wood is frequently distinguishable in them; whereas the Bovey coal comes out only in flat pieces, of a few feet long, like the splinters of large masts; and on them we discover no signs of roots, branches, or bark, no round pieces, or concentric circles, which distinguish the annual growth of trees; the laminæ, which have the appearance of wood, being always horizontal, according to the situation of the pieces in the strata: or could we suppose a number of fossil trees to be brought together, and ranged in this regular manner in the several strata,

strata, yet, by the form and roundness of their trunks, they must be, in a great measure, encompassed by the soil, in which they are buried; whereas there is not the least mixture of earth, or any other aperture, in the Bovey strata of coal, except a few crevices, common to this sort of fossil, which divide the pieces from each other in all directions, and seem to be inconsistent with the nature and fibrous texture of wood.

If the basis or matrix of this fossil were wood, it would acquire, by being impregnated with bitumen, a greater degree of inflammability; whereas it neither kindles nor consumes so fast as wood.

The inflammability and laminated texture of this fossil, which have been the only reasons for supposing it of vegetable origin, may be accounted for from the nature of its principles, and their disposition, when united, to assume certain forms. The bituminous matter uniting with various kinds of earth, in certain proportions, will form an inflammable substance; and a similar mixture of these will generally assume a similar texture and form, as will appear from the accounts given us of this fossil both by the ancients and moderns, most of whom speak of it as a mineral substance.

Theophrastus * mentions “ a stone on the promontory of Erineas in Peloponesus, and another “ in the mines of Binæ in Thrace, which being “ burnt, emitted a bituminous odour; and another “ stone in the mines of Scaptisulæ, not unlike to rotten

* De Lapidibus, p. 4.

“ wood, which would burn, if oil was poured on it,
 “ but would cease burning as soon as the oil was
 “ consumed.”

Dioscorides * observes of the lapis gagates, which is certainly a species of this fossil, “ that the sort, “ which kindles quickly, and gives a bituminous “ odour, is the best.” He describes it as generally of a black colour, and uneven substance, broad and flat, and very light. He mentions † also a black earth, found at Seleucia in Syria, called terra Ampe-litis, which resembled long pieces of fossil coal, splitting easily, of an uniform glossy superficies, which being pounded, and immersed in oil, immediately liquified.

Pliny ‡ describes the gagates as black, porous, and flat, not much unlike wood, brittle, of a disagreeable smell when rubbed, and emitting a sulphureous odour when burnt.

Galen || calls it “ a black stone, which, when put “ in the fire, sends forth a bituminous odour.” And he speaks of some other black stones, brought by him out of Cœlosyria, “ which were broad like a “ board, and, being put into the fire, burnt with a “ slender flame: they were generated in the hills on “ the east side of the Dead Sea, where the bitumen “ is produced, and the smell of the stone was like “ bitumen.” I cannot but observe on this passage, that the smell of the Bovey coal, when burnt, is the

* Lib. v. cap. 103.

† Lib. v. cap. 138.

‡ Hist. Nat. lib. xxxvi. cap. 19.

|| De Simplicium Medicamentorum Facultat. lib. ix. de Gagate.

same with that of the asphaltum, and of a stone impregnated with it, which is found at a small distance from the Dead Sea.

Agricola * mentions these, and several other kinds of bituminous fossils, taken notice of by the antients, and points out several places in Europe where they are now found. Later writers describe it under the name of metallophyton, or lignum fossile; and most of them suppose it to be a mineral substance. Francisco Stelluti, in a treatise printed at Rome 1637, has accurately described some very curious strata of this kind, near Todi and Aqua Sparta in Umbria: he says,
 “ they consist of large oval and compressed pieces,
 “ lying horizontally, resembling the trunks of trees,
 “ but larger: he mentions one above 3 feet broad:
 “ they have neither roots, branches, nor fibres, but
 “ something like bark; and the heart of the tree,
 “ which in some is as hard as a bone, in others as
 “ black as a coal. The extremity of these pieces is
 “ as smooth, as if they had been divided by a saw. It
 “ is full of veins of a dark brown colour, which, in-
 “ stead of running strait like the fibres of wood, are
 “ undulated and irregular, taking a variety of forms,
 “ and are only superficial; for, upon taking off one
 “ of the thin laminæ, of which this fossil is com-
 “ posed, the veins underneath appear in a different
 “ direction. This fossil, when first taken out of the
 “ earth, bends like a bow; and when put into the
 “ fire consumes slowly, with a great smoak, and dis-
 “ agreeable smell; but, if burnt when dry, the smell
 “ is more pleasing. The heat is more intense than

* De Natura Fossilium, lib. iv. p. 596.

“ wood,

“ wood, and it does not consume so fast. Many of
 “ these pieces are sprinkled with marcasite, and there
 “ exfudes from it a white bituminous substance, like
 “ rofin. It is full of crevices, in all directions, and the
 “ pieces appear in a variety of forms,” which the au-
 thor has exhibited, together with a representation of the
 veins of the fossil, in several copper-plates accompany-
 ing his work. He is clearly of opinion, “ that this
 “ substance is not generated from the seed or roots of
 “ any plant, but from a cretaceous earth transform-
 “ ing itself into wood, by the assistance of fulphu-
 “ reous water minerals, and subterraneous heat,
 “ which appears there in a thick smoak, and some-
 “ times in flames, particularly in rainy weather.”
 He forms his supposition on the different appear-
 ances of this fossil, “ some pieces of it being like
 “ chalk, others like wood, and again others like
 “ coal; some are either totally or partially petrified,
 “ being stone without and wood within, or *vice*
 “ *versa*.” But these various appearances may be
 easily accounted for, from the different kinds and
 proportions of earthy matter, with which the bitu-
 men is mixed.

Of the same kind are the fossil strata in Iceland,
 mentioned in the Musæum Wormianum, and by
 Horrebow, in his natural history of that island. The
 former of these authors * describes it as “ a laminated
 “ substance, generally of a black, but sometimes only
 “ of a dark colour, heavy, and, when dry, brittle;
 “ though the merchant, who gave the specimen,
 “ assured him, that when it was first dug out of the

* Lib. ii. cap. 16. p. 169.

“ earth

“ earth it would bend like a twig. It consists of
 “ oblique fibres, and here and there some knots
 “ like the roots of a great tree. A carpenter, who
 “ was a judge of wood, thought it to be the root
 “ of a walnut-tree grown black with age. It was
 “ not easily to be polished, too brittle to be used
 “ in cabinet work, and was full of crevices. This
 “ stratum is found some yards under the earth, in a
 “ mountain so high and perpendicular, that those
 “ only who have been accustomed to climb such
 “ precipices can venture to dig for it. There is not
 “ the least appearance, that trees ever grew where this
 “ fossil is found, notwithstanding this author sup-
 “ poses them to be roots of trees turned black, by a
 “ subterraneous vitriolic juice. The dust of this stra-
 “ tum is accounted, by the Icelanders, an excellent
 “ preservative of cloaths from moths and worms.”

Horrebow * says, “ it is an extraordinary sort of
 “ wood, which they call fortebrand, or black brand,
 “ very hard, heavy, and black like ebony. It is
 “ found deep in the ground, in broad, thin, and
 “ pretty large pannels, or leaves, fit for a moderate
 “ sized table. It is generally wavy, undulated ; and
 “ is always found between the rocks, or great stones,
 “ wedged, as it were, close in.” He was doubtful,
 whether it were wood, or a petrification ; but in-
 clined to the former opinion, because it could be
 planed and managed like wood.

The same sort of fossil, according to Wormius †,
 “ is found in the islands of Faro. It does not easily

* Page 33.

† Musæum Wormianum, p. 31. cap. 13. de Bitumine.

“ take fire, but has a splendor like gagates. It is
 “ found in the joints of the rock, and is taken out
 “ in laminæ, or splinters, of 3 or 4 inches thick.”

Of the like kind is the stratum of fossil wood near Thun in Swisserland, mentioned by Schentzer, in his *Itinera Alpina* *; which he describes as lying under several strata of flints, clay, and ash coloured marle. Being exposed to the air, parts of it grew hard, and others broke to pieces. “ It is (he says) “ observable of this fossil wood, that the trunks and “ branches are not round, but compressed; yet, in “ some places, cloathed with their bark, and here “ and there adorned with their leaves. The wood “ is inflammable, making a strong fire, and serves “ instead of fossil coals.” The author supposes the compression of this stratum to be owing to the great superincumbent weight; but others, he says, imagine it to be so formed by nature, from clay, in the bowels of the earth. I would observe here, that the crustaceous appearance, so common to these bituminous fossils, might easily be mistaken by this author for the bark of the tree, the same appearance being observed by Stelluti, in that of Umbria; but it is not so easy to account for the leaves, which certainly have no connection with a mineral substance.

John George Liebnecht, in his *Hassia subterranea*, quotes Pillingius's treatise on *bitumen* and *lignum fossile*, for the following description of a stratum of this latter kind. He says, that at Meisslibitz, in the duchy of Altenburg in Saxony, there is a mountain, with a gradual ascent and fertile summit, the

* Quarto, 1724. p. 604.

outward stratum of which is vegetable earth, three feet deep; and under it a stratum of clay, two fathom thick; then a stratum of sand, made blackish by bituminous fumes: *subtus lignum putridum, flammam concipiens, pectines annuos referens, ita à naturâ natum, ut nec aqua aquæ, nec lac lacti similis, quam ligno huic minerali lignum vegetabile putridum.*

Our English naturalists have likewise described this fossil, as found in several parts of the kingdom, and always in or near strata of clay.

Dr. Plott, in his natural history of Oxfordshire *, observes, “ that the scarcity of firing in some parts
“ of that county has induced people to burn a sort
“ of black substance, of a grain somewhat like rotten
“ wood half burnt, partaking also of a mineral na-
“ ture, and therefore by authors called metallophy-
“ ton, or lignum fossil. Put into water, it will not
“ swim; and into fire, it consumes but slowly, and
“ sends forth very unpleasant fumes. A vein of it
“ at Duchlington looked like wood; yet broken,
“ shewed a smooth and shining superficies, not un-
“ like to stone pitch; and put into the fire, has not
“ near so ill a smell. As to the substance of lignum
“ fossil, it is thought to be a cretaceous earth, turned
“ to what it is by subterraneous heats; for that it was
“ never formerly wood, notwithstanding its specious
“ and outward likeness, is plain from its never being
“ found with roots or boughs, or any other signs of
“ wood.” And, in his history of Staffordshire, he
quotes Scoochius’s opinion, “ that many of the
“ stumps and trunks of trees found in Holland, Zea-

* Page 65.

“ land, and Friezland, are of this mineral wood,
 “ chiefly because most of them are found without
 “ knots or roots.”

Dr. Morton, in his natural history of Northamp-
 tonshire *, mentions two or three varieties of this
 metallophyton; one of which he describes, as “ of
 “ a dark colour, and having a grain; for in one di-
 “ rection (which is usually according to the length
 “ of the pieces), it cleaves or parts pretty readily
 “ into plates and splinters, the other way it snaps
 “ into shorter pieces, and will not cleave at all.
 “ There is another sort, which does not so readily
 “ part into flakes. None of these are found in any
 “ large masses. They are all, more or less, of a
 “ glossy black, and have a density or smoothness within
 “ like that of bitumen, or jet. In that also, they
 “ resemble the true bitumen, or pissaphalton. They
 “ are not so firm and hard as the common coal, and
 “ are much brittler than canal or jet. They like-
 “ wise all of them agree pretty nearly in the same
 “ properties. In water they sink. They are all in-
 “ flammable, but consume slowly in the fire, and
 “ emit a somewhat unpleasant fume, not unlike that
 “ of bitumen, only fainter.”

Of the same kind is the inflammable slate, or coal,
 of Dorsetshire, found in the cliffs at the north-west
 corner of the isle of Portland, near the castle, of
 which I have received the following account from a
 gentleman of that county. The upper stratum is the
 natural black earth 14 feet deep; the next is a greyish,
 soft, paving stone, 6 inches thick; then follows the vein

of coal, of 10 inches; under which is a black earth, and at the bottom of the clift, clay. It riles in laminæ, and is discovered in the outermost part of the clift, whence it is taken, by digging horizontally: some is black, and some of a redish colour; which latter is reputed the worst, and does not burn so well. The ashes of both are reckoned very good manure for clay ground. It is made use of to heat ovens; and when piled up as turff, gives a clear light, and yields a strong bituminous smell. It is found more to the west, at Chickerel and Fleet, in the sands, the shore being open.

The Kimendge coal, which, in all respects, resembles that before-mentioned, and is so called from the place where it is dug, appears in most of the cliffs of the isle of Purbeck, from St. Aldhelm's chapel to East-Lulworth, and at Osmington, opposite to that part of Portland where the coal is dug. It is found 16 feet below the top of the rock, in great lumps; but the stratum does not exceed 2 feet in thickness, nor extend far from the shore. It emits, when burnt, a strong sulphureous smell, and has a great many yellow strokes and spots on it, which seem to be marcasite. It is almost wholly used by the poor, in their ovens and chimneys, and is sold for nine pence *per* hogshead, or six shillings *per* ton. Under water, and in the cliffs, it is very hard; but when exposed to the air it shivers into pieces like slate.

At Kimendge are found small round pieces of this coal, from one to three inches in diameter, one side of which is flat, the other tapering, like the lower part of a cone. In some there is a square hole in

the center on the flat side; in most of them two, or sometimes four holes, near the edge, but they do not penetrate quite through the piece. They are sometimes found at the top of the cliffs, between two stones set on edge, and covered with a third, mingled with human bones. Sometimes they are found by themselves, but always in made ground. The common people call them coal money. I think it can scarce be doubted, that they are British antiquities.

To these, I shall add Dr. Hoffman's * account of a stratum of the same kind in Prussia, in the neighbourhood of Fischhausen, &c. where they dig for amber. "The upper stratum is sand, under it a
" bed of clay, and then a woody stratum, consisting
" of a substance like old wood, but inflammable;
" under this was a vitriolic mineral; and, lastly, a
" bed of sand, in which a great quantity of amber was
" found." There is a remarkable resemblance between the order of these strata, and those at Bovey; and though no amber has been discovered at the latter place, yet it would seem, as if there were some connection between that and these bituminous strata; for it is remarkable, that Theophrastus †, speaking of an earthy inflammable substance in Liguria of this kind, adds, that amber is found there.

The resemblance likewise, which the strata of Munden and Allendorf bear to those of Bovey, is remarkable, the soil in which they lie consisting of clays, boles, and sand. This is so much the case at Bovey, that they have large beds of a very fine pipe

* Observationes Physico-chemicals, lib. ii. obs. 23. p. 199.

† De Lapidibus, p. 4.

clay in the neighbourhood, which has for many years been exported to Liverpool, and manufactured there into earthen ware; but there is one now erected at Bovey. The depth of the strata is likewise much the same; for at Munden they have sunk 50 feet, without coming to the bottom. The pieces, that form these strata, seem to lie much in the same manner; they are compressed, without any intermixture of earth, full of cracks and fissures, and lying so close upon each other, that the professor says, “it could not be discovered, whether they were entire trees, or only parts of them,” and when taken out, they easily fell to pieces. The German strata resemble those of Bovey in colour. That of Allendorf, which Dr. Hollman calls bitumen, and which he distinguishes from the fossil wood lying under it, is said to be little inferior in blackness, splendor, and hardness, to jet; and he adds, that the fossil wood approaches very nearly to the same colour. He observes, indeed, that the stratum of Allendorf is not so strongly impregnated with bitumen, and therefore left by the workmen for future ages; and, no doubt, some species of this fossil have so much earth, and so little bitumen, that they are not at all, or only in a small degree, inflammable. I shall only observe further, that the layer of stone, which divides the two strata in the quarry of Munden, is a strong objection to the notion of their being fossil wood, properly so called; and that the vein of the stratum at Allendorf, being taken in the adit of a mine, could not give an opportunity of examining the nature and disposition of its parts, with the same advantage, that we can view the strata at Bovey.

I shall

I shall conclude this paper with observing the several particulars, in which all the species of the bituminous fossils before-mentioned resemble each other. They seem to be generally found between beds of clay, or stone; are of a dark brown, or black colour, of a laminated texture: pliable when moist, and fresh dug, but crisp and brittle when dry; full of cracks, and easily breaking transversely; they all sink in water, and emit the same nauseous and bituminous smell; they differ in being more or less solid, heavy, and inflammable, according to the proportions and principles of which they consist; and if any doubt could remain of their being a mineral substance, it must be removed by the following analysis.

One pound of Bovey coal, of the woody kind, powdered, put into a glass retort, and distilled in sand, yielded four ounces and a half of phlegm, which had the appearance of common water, but somewhat of a bituminous smell and taste; near four ounces of a turbid whitish bituminous liquor, of an intolerable foetid smell, and extremely pungent to the tongue; about two drachms of a heavy bituminous matter, which would not mix with the liquor above-mentioned, but sunk entirely to the bottom, and (which is very remarkable) there was not the least appearance of any light oil floating on the bituminous liquor. There remained in the retort about seven ounces of a very black powder, which had the same bituminous smell, not very heavy; some of which being put on a red-hot iron, emitted a little sinoak, but no flame.

The

The ashes of this fossil, when burnt, being boiled in water, and the water evaporated, there remained no salt behind.

I am, my Lord, &c.

Grosvenor-Street, Feb. 28, 1760.

LIV. *A new Method of computing the Sums of certain Series; by Mr. John Landen; Communicated by Mr. Thomas Simpson, F. R. S.*

Read Feb. 28,
1760.

AS the improving the analytic art, especially any branch of it that relates to the summation of series, may, by facilitating computations, conduce to the improvement of several branches of science; it is presumed, that this paper, which exhibits a new and easy method of computing the sums of a great number of infinite series, may be acceptable to the mathematical world, and deemed worthy to be inserted in the British Philosophical Transactions.

I.

Supposing x to be the sine of the circular arc z , whose radius is 1, $\frac{z}{\sqrt{1-x^2}}$ will be $= z$; and, consequently, $\frac{z}{\sqrt{x^2-1}} = \frac{z}{\sqrt{-1}}$. From whence, by taking the correct fluents, we have hyp. log.

$$\frac{x + \sqrt{x^2 - 1}}{\sqrt{-1}} = \frac{z}{\sqrt{-1}}.$$

Hence,

Hence, writing a for one fourth of the periphery of the circle whose radius is 1, and taking x equal to the said radius, we find hyp. log. $\frac{1}{\sqrt{-1}} = \frac{a}{\sqrt{-1}}$; and, consequently, hyp. log. $\sqrt{-1} = \frac{-a}{\sqrt{-1}}$, and hyp. log. $-1 = \pm \frac{2a}{\sqrt{-1}}$.

2.

The hyp. log. of $\frac{1}{1-x}$ being $= x + \frac{x^2}{2} + \frac{x^3}{3} + \frac{x^4}{4}$, &c.
 $\dot{\text{F}}^{\text{I}} =$ fluent of $\frac{\dot{x}}{x}$ hyp. log. $\frac{1}{1-x}$, is $= x + \frac{x^2}{2^2} + \frac{x^3}{3^2}$
 $+ \frac{x^4}{4^2}$, &c.

$\dot{\text{F}}^{\text{II}} =$ fluent of $\frac{\dot{x}}{x} \dot{\text{F}}^{\text{I}} = x + \frac{x^2}{2^3} + \frac{x^3}{3^3} + \frac{x^4}{4^3}$, &c.

$\dot{\text{F}}^{\text{III}} =$ fluent of $\frac{\dot{x}}{x} \dot{\text{F}}^{\text{II}} = x + \frac{x^2}{2^4} + \frac{x^3}{3^4} + \frac{x^4}{4^4}$, &c.

$\dot{\text{F}}^{\text{IV}} =$ fluent of $\frac{\dot{x}}{x} \dot{\text{F}}^{\text{III}} = x + \frac{x^2}{2^5} + \frac{x^3}{3^5} + \frac{x^4}{4^5}$, &c.
 &c. &c. &c.

3.

By writing, in the first equation in the preceding article, $\frac{1}{x}$ instead of x , we have

$$\text{Hyp. log. } \frac{1}{1 - \frac{1}{x}} = x^{-1} + \frac{x^{-2}}{2} + \frac{x^{-3}}{3}, \text{ \&c.}$$

But the hyp. log. of $\frac{1}{1 - \frac{1}{x}}$ is $=$ hyp. log. $\frac{x}{x-1} =$
 hyp.

hyp. log. $\frac{1}{1-x} + \text{hyp. log. } x + \text{hyp. log. } -1 =$
 $\pm 2b + X + \text{hyp. log. } \frac{1}{1-x}$, b being put for $\frac{a}{\sqrt{-1}}$,
 and X for the hyp. log. of x .

It is evident, therefore, that

Hyp. log. $\frac{1}{1-x}$ is $= \mp 2b - X + x^{-1} + \frac{x^{-2}}{2} + \frac{x^{-3}}{3}$, &c.
 where, of the two signs prefixed to $2b$, the upper
 one takes place, when the hyp. log. of -1 is taken
 equal to $\frac{-2a}{\sqrt{-1}}$, likewise when x is taken equal to
 $\sqrt{-1}$; and the lower one takes place, when the
 hyp. log. of -1 is taken equal to $\frac{2a}{\sqrt{-1}}$, also when
 x is taken equal to $\frac{1}{\sqrt{-1}}$: wherefore, if we observe
 to take the value of hyp. log. of -1 , as last men-
 tioned, and x equal to $\frac{1}{\sqrt{-1}}$, instead of $\sqrt{-1}$, we
 need retain only the lower of the said signs.

4.

For brevity sake, we shall, in what follows, put

the series $1 + \frac{1}{2^2} + \frac{1}{3^2} + \frac{1}{4^2}$, &c. $= \overset{\text{II}}{P}$,

$1 + \frac{1}{2^4} + \frac{1}{3^4} + \frac{1}{4^4}$, &c. $= \overset{\text{IV}}{P}$,

$1 + \frac{1}{2^6} + \frac{1}{3^6} + \frac{1}{4^6}$, &c. $= \overset{\text{VI}}{P}$,

&c. &c.

$1 + \frac{1}{3^2} + \frac{1}{5^2} + \frac{1}{7^2}$, &c. $= \overset{\text{II}}{Q}$

$$1 - \frac{1}{3^3} + \frac{1}{5^3} - \frac{1}{7^3} +, \mathcal{E}c. = q, \quad \text{III}$$

$$1 + \frac{1}{3^4} + \frac{1}{5^4} + \frac{1}{7^4}, \mathcal{E}c. = Q, \quad \text{IV}$$

$$1 - \frac{1}{3^5} + \frac{1}{5^5} - \frac{1}{7^5} +, \mathcal{E}c. = q, \quad \text{V}$$

$\mathcal{E}c.$

$\mathcal{E}c.$

5.

Multiplying the last equation in art. 3. by $\frac{\dot{x}}{x}$, and taking the correct fluents, we have

$$\dot{F} = 2 \ddot{P} + 2 b X - \frac{X^2}{2} - x^{-1} - \frac{x^{-2}}{2^2} - \frac{x^{-3}}{3^2}, \mathcal{E}c.$$

From whence, by multiplying by $\frac{\dot{x}}{x}$, and taking the fluents, we get

$$\ddot{F} = 2 \ddot{P} X + b X^2 - \frac{X^3}{2 \cdot 3} + x^{-1} + \frac{x^{-2}}{2^3} + \frac{x^{-3}}{3^3}, \mathcal{E}c.$$

Again, multiplying the last equation by $\frac{\dot{x}}{x}$, and taking the correct fluents, we find

$$\ddot{\ddot{F}} = 2 \ddot{\ddot{P}} + \ddot{P} X^2 + \frac{b X^3}{3} - \frac{X^4}{2 \cdot 3 \cdot 4} - x^{-1} - \frac{x^{-2}}{2^4} - \frac{x^{-3}}{3^4}, \mathcal{E}c.$$

And, by proceeding in the same manner, we find

$$\ddot{\ddot{\ddot{F}}} = 2 \ddot{\ddot{\ddot{P}}} X + \frac{\ddot{\ddot{P}} X^3}{3} + \frac{b X^4}{3 \cdot 4} - \frac{X^5}{2 \cdot 3 \cdot 4 \cdot 5} + x^{-1} + \frac{x^{-2}}{2^5} + \frac{x^{-3}}{3^5}, \mathcal{E}c.$$

$\mathcal{E}c.$

$\mathcal{E}c.$

6.

Now, it is obvious, that $x + \frac{x^2}{2^2} + \frac{x^3}{3^2}, \mathcal{E}c.$ the value of $\dot{F}$ in art. 2. must be equal to $2 \ddot{P} + 2 b X$

$-\frac{X^2}{2} - x^{-1} - \frac{x^{-2}}{2^2} - \frac{x^{-3}}{3^2}$, &c. the value of $\bar{P}$ in art. 5. when both series converge.

Therefore, $\frac{x + x^{-1}}{1^2} + \frac{x^2 + x^{-2}}{2^2} + \frac{x^3 + x^{-3}}{3^2}$, &c. is then $= 2 \bar{P} + 2 b X - \frac{X^2}{2}$.

From which equation, by taking x equal to -1 , we have $-\frac{1}{1^2} + \frac{1}{2^2} - \frac{1}{3^2} + \frac{1}{4^2} -$, &c. $= \bar{P} + b^2 = \bar{P} - a^2$; and, by taking x equal to $\frac{1}{\sqrt{-1}}$, we have

$-\frac{1}{1^2} + \frac{1}{2^2} - \frac{1}{3^2} + \frac{1}{4^2} -$, &c. $= 4 \bar{P} + 3 b^2 = 4 \bar{P} - 3 a^2$.

Therefore $4 \bar{P} - 3 a^2$ is $= \bar{P} - a^2$;

Hence $\bar{P}$ is found $= \frac{2 a^2}{3}$.

Moreover $\frac{1}{1^2} + \frac{1}{2^2} + \frac{1}{3^2} + \frac{1}{4^2}$, &c. being $= \bar{P}$, by supposition; and $-\frac{1}{1^2} + \frac{1}{2^2} - \frac{1}{3^2} + \frac{1}{4^2} -$, &c. $= \bar{P} - a^2$, as found above; we, by subtraction, get $\frac{2}{1^2} + \frac{2}{3^2} + \frac{2}{5^2}$, &c. $(= 2 \bar{Q}) = a^2$, and, consequently $\bar{Q} = \frac{a^2}{2}$.

SCHOLIUM.

The hyp. log. of $\frac{1}{1-x}$ being $= x + \frac{x^2}{2} + \frac{x^3}{3}$, &c. we, by writing $1 - x$ instead of x , have

4 C 2

Hyp.

Hyp. log. of $\frac{1}{x} = 1 - x + \frac{1-x^2}{2} + \frac{1-x^3}{3}$, $\mathcal{E}c.$

and consequently $X = -1 - x - \frac{1-x^2}{2} - \frac{1-x^3}{3}$, $\mathcal{E}c.$

Moreover the fluent of $\frac{\dot{x}}{x} \times \text{hyp. log. of } \frac{1}{1-x}$ is $= x + \frac{x^2}{2^2} + \frac{x^3}{3^2}$, $\mathcal{E}c.$ which vanishes when x vanishes;

and the fluent of $\frac{\dot{x}}{1-x} \times X$ is $= \frac{1-x}{1-x} + \frac{1-x^2}{2^2} + \frac{1-x^3}{3^2}$, $\mathcal{E}c.$ $-\ddot{P}$, being corrected so as to vanish when x vanishes.

But the fluent of $\frac{\dot{x}}{x} \times \text{hyp. log. of } \frac{1}{1-x} + \text{fluent of } \frac{\dot{x}}{1-x} \times X$ is $= X \times \text{hyp. log. of } \frac{1}{1-x}$, which also vanishes when x vanishes.

Therefore $X \times \text{hyp. log. of } \frac{1}{1-x}$ is $= x + \frac{x^2}{2^2} + \frac{x^3}{3^2}$, $\mathcal{E}c.$ $+ 1 - x + \frac{1-x^2}{2^2} + \frac{1-x^3}{3^2}$, $\mathcal{E}c.$ $-\ddot{P}$.

From whence, by taking x equal to $\frac{1}{2}$, we find $-\text{square of hyp. log. of } 2 = 2 \times \frac{1}{1^2 \cdot 2^1} + \frac{1}{2^2 \cdot 2^2} + \frac{1}{3^2 \cdot 2^3}$, $\mathcal{E}c.$ $-\ddot{P}$: hence, $\ddot{P}$ being before found $= \frac{2a^2}{3}$, it appears that, when x is $= \frac{1}{2}$, the series $x + \frac{x^2}{2^2} + \frac{x^3}{3^2}$, $\mathcal{E}c.$ is $= \frac{a^2}{3} - \frac{1}{2} \times \text{hyp. log. of } 2^2$.

7.

Furthermore, $x + \frac{x^2}{2^2} + \frac{x^3}{3^2}$, $\mathcal{E}c.$ the value of $\ddot{F}$ in art. 2. must be equal to $2\ddot{P}X + bX^2 - \frac{X^3}{2 \cdot 3} + x^{-1}$
 $+$

$+ \frac{x^{-2}}{2^3} + \frac{x^{-3}}{3^3}$, &c. the value of $\overset{\text{II}}{F}$ in art. 5. when both series converge.

Therefore $\frac{x - x^{-1}}{1^3} + \frac{x^2 - x^{-2}}{2^3} + \frac{x^3 - x^{-3}}{3^3}$, &c. is then $= 2 \overset{\text{II}}{P} X + b X^2 - \frac{X^3}{2.3}$.

From whence, by taking x equal to -1 , we have $4b \overset{\text{II}}{P} + 4b^3 - \frac{8b^3}{2.3} = 0$; and, consequently, $\overset{\text{II}}{P} = \frac{2a^2}{3}$, as before found.

And, by taking x equal to $\frac{1}{\sqrt{-1}}$, we find

$$\frac{2}{\sqrt{-1}} \times \overset{\text{III}}{q} = 2b \overset{\text{II}}{P} + b^3 - \frac{b^3}{2.3} = \frac{4a^3}{3\sqrt{-1}} - \frac{a^3}{\sqrt{-1}} + \frac{a^3}{2.3\sqrt{-1}} = \frac{a^3}{2\sqrt{-1}}.$$

Therefore $\overset{\text{III}}{q}$ is $= \frac{a^3}{4}$.

8.

From what is done above, it evidently follows, that

$$-\overset{\text{IV}}{P} \text{ is } = \frac{2b^2 \overset{\text{II}}{P}}{3} + \frac{2.8b^4}{3.4.5},$$

$$-\overset{\text{VI}}{P} = \frac{2b^2 \overset{\text{IV}}{P}}{3} + \frac{8b^4 \overset{\text{II}}{P}}{3.4.5} + \frac{3.32b^6}{3.4.5.6.7},$$

&c.

&c.

$$-\overset{\text{IV}}{Q} = b^2 \overset{\text{II}}{P} + \frac{3.2b^4}{3.4},$$

$$-\overset{\text{VI}}{Q} = b^2 \overset{\text{IV}}{P} + \frac{4b^4 \overset{\text{II}}{P}}{3.4} + \frac{5.8b^6}{3.4.5.6},$$

&c.

&c.

$$\begin{aligned} \frac{q^{\text{v}}}{\sqrt{-1}} \text{ is } &= b^{\text{iv}} \bar{P} + \frac{b^3 \bar{P}^{\text{ii}}}{2 \cdot 3} + \frac{9 b^5}{2 \cdot 2 \cdot 3 \cdot 4 \cdot 5}, \\ \frac{q^{\text{vii}}}{\sqrt{-1}} &= b^{\text{vi}} \bar{P} + \frac{b^3 \bar{P}^{\text{iv}}}{2 \cdot 3} + \frac{b^5 \bar{P}^{\text{ii}}}{2 \cdot 3 \cdot 4 \cdot 5} + \frac{13 b^7}{2 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6 \cdot 7}, \\ \mathcal{E}c. & \qquad \qquad \qquad \mathcal{E}c. \end{aligned}$$

From whence the values of $\bar{P}^{\text{iv}}$, $\bar{P}^{\text{vi}}$, $\mathcal{E}c.$ $\bar{Q}^{\text{iv}}$, $\bar{Q}^{\text{vi}}$, $\mathcal{E}c.$ q^{v} , q^{vii} , $\mathcal{E}c.$ may be easily obtained, in terms of a .

9.

$$\begin{aligned} \text{Hyp. log. } \frac{1+x^{\frac{1}{2}}}{1-x} \text{ being } &= x + \frac{x^3}{3} + \frac{x^5}{5}, \mathcal{E}c. \\ \bar{G}^{\text{i}} = \text{fluent of } \frac{\dot{x}}{x} \text{ hyp. log. } \frac{1+x^{\frac{1}{2}}}{1-x} \text{ is } &= x + \frac{x^3}{3^2} + \frac{x^5}{5^2}, \mathcal{E}c. \\ \bar{G}^{\text{ii}} = \text{fluent of } \frac{\dot{x}}{x} \bar{G}^{\text{i}} &= x + \frac{x^3}{3^3} + \frac{x^5}{5^3}, \mathcal{E}c. \\ \bar{G}^{\text{iii}} = \text{fluent of } \frac{\dot{x}}{x} \bar{G}^{\text{ii}} &= x + \frac{x^3}{3^4} + \frac{x^5}{5^4}, \mathcal{E}c. \\ \mathcal{E}c. & \qquad \mathcal{E}c. \qquad \qquad \mathcal{E}c. \end{aligned}$$

10.

By writing, in the first equation in the preceding article, $\frac{1}{x}$ instead of x , we have

$$\text{Hyp. log. } \frac{1+\frac{1}{x^{\frac{1}{2}}}}{1-\frac{1}{x}} = x^{-1} + \frac{x^{-3}}{3} + \frac{x^{-5}}{5}, \mathcal{E}c.$$

$$\begin{aligned} \text{But the hyp. log. of } \frac{1+\frac{1}{x^{\frac{1}{2}}}}{1-\frac{1}{x}} \text{ is } &= \text{hyp. log. } \frac{x+1}{x-1} \\ &= \text{hyp. log. } \frac{1+x^{\frac{1}{2}}}{1-x} + \text{hyp. log. } \sqrt{-1} = \pm b + \\ &\text{hyp. log. } \frac{1+x^{\frac{1}{2}}}{1-x}. \end{aligned}$$

It

It is manifest, therefore, that

$$\text{Hyp. log. } \frac{1+x}{1-x}^{\frac{1}{2}} \text{ is } = \mp b + x^{-1} + \frac{x^{-3}}{3} + \frac{x^{-5}}{5}, \text{ \&c.}$$

where, with respect to the two signs prefixed to b , the same observation may be made as in art. 3.

II.

Multiplying the last equation by $\frac{\dot{x}}{x}$, and taking the correct fluents, we have

$$\dot{G} = 2 \dot{Q} + bX - x^{-1} - \frac{x^{-3}}{3^2} - \frac{x^{-5}}{5^2}, \text{ \&c.}$$

From whence, by multiplying by $\frac{\dot{x}}{x}$, and taking the fluents, we get

$$\ddot{G} = 2 \ddot{Q}X + \frac{bX^2}{2} + x^{-1} + \frac{x^{-3}}{3^3} + \frac{x^{-5}}{5^3}, \text{ \&c.}$$

Again, multiplying the last equation by $\frac{\dot{x}}{x}$, and taking the correct fluents, we find

$$\ddot{G} = 2 \ddot{Q} + \ddot{Q}X^2 + \frac{bX^3}{2.3} - x^{-1} - \frac{x^{-3}}{3^4} - \frac{x^{-5}}{5^4}, \text{ \&c.}$$

And, by proceeding in the same manner, we find

$$\ddot{G} = 2 \ddot{Q}X + \frac{\ddot{Q}X^3}{3} + \frac{bX^4}{2.3.4} + x^{-1} + \frac{x^{-3}}{3^5} + \frac{x^{-5}}{5^5}, \text{ \&c.}$$

12.

Now, it is obvious, that $x + \frac{x^3}{3^2} + \frac{x^5}{5^2}$, \&c. the value of $\dot{G}$ in art. 9. must be equal to $2 \dot{Q} + bX - x^{-1} - \frac{x^{-3}}{3^2} - \frac{x^{-5}}{5^2}$, \&c. the value of $\dot{G}$ in art. 11. when both series converge.

There-

Therefore $\frac{x + x^{-1}}{1^2} + \frac{x^3 + x^{-3}}{3^2} + \frac{x^5 + x^{-5}}{5^2}$, $\mathcal{E}c.$ is then $= 2 \overset{II}{Q} + bX$.

From whence, by taking x equal to $\frac{1}{\sqrt{-1}}$, we have $2 \overset{II}{Q} + b^2 = 0$; and, consequently, $\overset{II}{Q} = \frac{a^2}{2}$, as in art. 6.

13.

Likewise $x + \frac{x^3}{3^2} + \frac{x^5}{5^2}$, $\mathcal{E}c.$ the value of $\overset{II}{G}$ in art. 9. must be equal to $2 \overset{II}{Q}X + \frac{bX^2}{2} + x^{-1} + \frac{x^{-3}}{3^2} + \frac{x^{-5}}{5^2}$, $\mathcal{E}c.$ the value of $\overset{II}{G}$ in art. 11. when both series converge.

Therefore $\frac{x - x^{-1}}{1^3} + \frac{x^3 - x^{-3}}{3^3} + \frac{x^5 - x^{-5}}{5^3}$, $\mathcal{E}c.$ is then $= 2 \overset{II}{Q}X + \frac{bX^2}{2}$.

Hence, by taking $x = \frac{1}{\sqrt{-1}}$, we find $\frac{2}{\sqrt{-1}} \times \overset{III}{q} = 2b \overset{II}{Q} + \frac{b^2}{2} = \frac{a^3}{2\sqrt{-1}}$; and, consequently, $\overset{III}{q} = \frac{a^3}{4}$, as in art. 7.

14.

From what is done in the last five articles, it evidently follows, that

$$\begin{aligned} - \overset{IV}{Q} \text{ is } &= \frac{b^2 \overset{II}{Q}}{2} + \frac{b^4}{2.2.3}, \\ \frac{\overset{V}{q}}{\sqrt{-1}} &= b \overset{IV}{Q} + \frac{b^3 \overset{II}{Q}}{2.3} + \frac{b^5}{2.2.3.4}, \end{aligned}$$

$$\begin{aligned} -\overset{\text{VI}}{Q} \text{ is} &= \frac{b^2 \overset{\text{IV}}{Q}}{2} + \frac{b^4 \overset{\text{II}}{Q}}{2 \cdot 3 \cdot 4} + \frac{b^6}{2 \cdot 2 \cdot 3 \cdot 4 \cdot 5}, \\ \frac{\overset{\text{VII}}{q}}{\sqrt{-1}} &= b \overset{\text{VI}}{Q} + \frac{b^3 \overset{\text{IV}}{Q}}{2 \cdot 3} + \frac{b^5 \overset{\text{II}}{Q}}{2 \cdot 3 \cdot 4 \cdot 5} + \frac{b^7}{2 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6}, \\ \mathcal{E}c. & \qquad \qquad \mathcal{E}c. \end{aligned}$$

From whence (as well as from the theorems in art. 8.) may the values of $\overset{\text{IV}}{Q}$, $\overset{\text{V}}{q}$, $\overset{\text{VI}}{Q}$, $\overset{\text{VII}}{q}$, $\mathcal{E}c.$ be readily found, in terms of a .

15.

$\overset{\text{I}}{G}$ being $= x + \frac{x^3}{3^2} + \frac{x^5}{5^2}$, $\mathcal{E}c.$ by art. 9.

$\overset{\text{I}}{H}$ = fluent of $x \dot{\overset{\text{I}}{G}}$ is $= \frac{x^3}{1^2 \cdot 3} + \frac{x^5}{3^2 \cdot 5} + \frac{x^7}{5^2 \cdot 7}$, $\mathcal{E}c.$

$\overset{\text{II}}{H}$ = fluent of $\dot{\overset{\text{I}}{H}}$ $= \frac{x^3}{1^2 \cdot 3^2} + \frac{x^5}{3^2 \cdot 5^2} + \frac{x^7}{5^2 \cdot 7^2}$, $\mathcal{E}c.$

$\overset{\text{III}}{H}$ = fluent of $x \dot{\overset{\text{II}}{H}}$ $= \frac{x^5}{1^2 \cdot 3^2 \cdot 5} + \frac{x^7}{3^2 \cdot 5^2 \cdot 7} + \frac{x^9}{5^2 \cdot 7^2 \cdot 9}$, $\mathcal{E}c.$

$\overset{\text{IV}}{H}$ = fluent of $\dot{\overset{\text{III}}{H}}$ $= \frac{x^5}{1^2 \cdot 3^2 \cdot 5^2} + \frac{x^7}{3^2 \cdot 5^2 \cdot 7^2} + \frac{x^9}{5^2 \cdot 7^2 \cdot 9^2}$, $\mathcal{E}c.$

16.

Moreover, $\overset{\text{I}}{G}$ being $= 2 \overset{\text{II}}{Q} + bX - x^{-1} - \frac{x^{-3}}{3^2} - \frac{x^{-5}}{5^2}$, $\mathcal{E}c.$ by art. 11. we, by multiplying by $x \dot{}$, and taking the correct fluents, get $\overset{\text{I}}{H} = x^2 \overset{\text{II}}{Q} - \overset{\text{II}}{Q} + \frac{b x^2 X}{2} - \frac{b x^2}{4} + \frac{b}{4} - x + 1 + 2 \overset{\text{II}}{S} + \frac{x^{-1}}{1 \cdot 3^2} + \frac{x^{-3}}{3 \cdot 5^2} + \frac{x^{-5}}{5 \cdot 7^2}$, $\mathcal{E}c.$ $\overset{\text{II}}{S}$ being put for the series $\frac{\overset{\text{I}}{Q}}{1^2 \cdot 3^2} + \frac{\overset{\text{I}}{Q}}{3^2 \cdot 5^2} + \frac{\overset{\text{I}}{Q}}{5^2 \cdot 7^2}$, $\mathcal{E}c.$

Now, it is obvious, that this value of $\overset{I}{H}$ must be equal to the value of $\overset{I}{H}$ in the preceding article, when both series converge.

Therefore $\frac{3x^3 - x^{-1}}{1^2 \cdot 3^2} + \frac{5x^5 - 3x^{-3}}{3^2 \cdot 5^2} + \frac{7x^7 - 5x^{-5}}{5^2 \cdot 7^2},$
 $\mathcal{E}^2 c.$ is then $= x^2 \overset{II}{Q} - \overset{II}{Q} + \frac{bx^2 X}{2} - \frac{bx^2}{4} + \frac{b}{4} - x$
 $+ 1 + 2 \overset{II}{S}.$

Hence, by taking x equal to -1 , we find $-2 \overset{II}{S}$
 $= b^2 + 2 + 2 \overset{II}{S};$ and, consequently, $\overset{II}{S} = \frac{a^2}{4} - \frac{1}{2}.$

And, by taking x equal to $\frac{1}{\sqrt{-1}}$, we find

$$-\frac{2}{\sqrt{-1}} \times \frac{1}{1^2 \cdot 3^2} - \frac{1}{3^2 \cdot 5^2} + \frac{1}{5^2 \cdot 7^2} - , \mathcal{E}^2 c. = -2 \overset{II}{Q}$$

$$- \frac{b^2}{2} + \frac{b}{2} - \frac{1}{\sqrt{-1}} + 1 + 2 \overset{II}{S} = \frac{a}{2\sqrt{-1}} - \frac{1}{\sqrt{-1}};$$

and, consequently, $\frac{1}{1^2 \cdot 3^2} - \frac{1}{3^2 \cdot 5^2} + \frac{1}{5^2 \cdot 7^2} - , \mathcal{E}^2 c. =$
 $\frac{1}{2} - \frac{a}{4}.$

17.

Seeing that $\overset{II}{Q}$ is $= \frac{a^2}{2}$, and $\overset{II}{S} = \frac{a^2}{4} - \frac{1}{2}$, it follows, from the last article, that

$$\overset{I}{H} \text{ is } = x^2 \overset{II}{Q} + \frac{bx^2 X}{2} - \frac{bX^2}{4} + \frac{b}{4} - x + \frac{x^{-1}}{1 \cdot 3^2}$$

$$+ \frac{x^{-3}}{3 \cdot 5^2} + \frac{x^{-5}}{5 \cdot 7^2}, \mathcal{E}^2 c.$$

From whence, by multiplying by $\frac{x}{x}$, and taking the correct fluent, we get

$$\begin{aligned} \text{H}^{\text{II}} &= \frac{x^2 \text{Q}}{2} + \frac{b x^2 \text{X}}{4} - \frac{b x^2}{4} + \frac{b}{4} + \frac{b \text{X}}{4} - x + \frac{a^2}{4} \\ &- \frac{x^{-1}}{1^2 \cdot 3^2} - \frac{x^{-3}}{3^2 \cdot 5^2} - \frac{x^{-5}}{5^2 \cdot 7^2}, \text{ \&C.} \end{aligned}$$

And hence, by multiplying by x^* , and taking the correct fluents, we have

$$\begin{aligned} \text{H}^{\text{III}} &= \frac{x^4 \text{Q}}{8} + \frac{b x^4 \text{X}}{16} - \frac{5 b x^4}{64} + \frac{b x^2}{16} + \frac{b x^2 \text{X}}{8} + \frac{b}{64} \\ &- \frac{x^2}{3} - \frac{x}{9} + \frac{4}{9} + \frac{a^2 x^2}{8} - \frac{3 a^2}{16} + 4 \text{S}^{\text{III}} + \frac{x^{-1}}{1 \cdot 3^2 \cdot 5^2} \\ &+ \frac{x^{-3}}{3 \cdot 5^2 \cdot 7^2} + \frac{x^{-5}}{5 \cdot 7^2 \cdot 9^2}, \text{ \&C. } \text{S}^{\text{III}} \text{ being put for the series} \\ &\frac{\text{I}}{1^2 \cdot 3^2 \cdot 5^2} + \frac{\text{I}}{3^2 \cdot 5^2 \cdot 7^2} + \frac{\text{I}}{5^2 \cdot 7^2 \cdot 9^2}, \text{ \&C.} \end{aligned}$$

Now, this value of H being equal to the value of H in art. 15. when both series converge, it follows, that

$$\begin{aligned} &\frac{5 x^5 - x^{-1}}{1^2 \cdot 3^2 \cdot 5^2} + \frac{7 x^7 - 3 x^{-3}}{3^2 \cdot 5^2 \cdot 7^2} + \frac{9 x^9 - 5 x^{-5}}{5^2 \cdot 7^2 \cdot 9^2}, \text{ \&C. is then} \\ &= \frac{x^4 \text{Q}}{8} + \frac{b x^4 \text{X}}{16} - \frac{5 b x^4}{64} + \frac{b x^2}{16} + \frac{b x^2 \text{X}}{8} + \frac{b}{64} - \frac{x^3}{3} \\ &- \frac{x}{9} + \frac{4}{9} + \frac{a^2 x^2}{8} + \frac{3 a^2}{16} - 4 \text{S}^{\text{III}}. \end{aligned}$$

Hence, by taking x equal to -1 , we find -4S^{III}
 $= \frac{3 b^2}{8} + \frac{8}{9} + 4 \text{S}^{\text{III}}$; and, consequently, $\text{S}^{\text{III}} = \frac{3 a^2}{64} - \frac{1}{9}$.

Many other instances of the use of this method might be given; but these may suffice to enable the intelligent reader to pursue the speculation farther, at his pleasure.

LV. *Conjectures concerning the Cause, and Observations upon the Phænomena of Earthquakes; particularly of that great Earthquake of the First of November, 1755, which proved so fatal to the City of Lisbon, and whose Effects were felt as far as Africa, and more or less throughout almost all Europe; by the Reverend John Michell, M. A. Fellow of Queen's College, Cambridge.*

INTRODUCTION.

Read Feb. 28.
March 6. 13.
20. 27. 1760.

ART. I. **I**T has been the general opinion of philosophers, that earthquakes owe their origin to some sudden explosion in the internal parts of the earth. This opinion is very agreeable to the phænomena, which seem plainly to point out something of that kind. The conjectures, however, concerning the cause of such an explosion, have not been yet, I think, sufficiently supported by facts; nor have the more particular effects, which will arise from it, been traced out; and the connexion of them with the phænomena explained. To do this, is the intent of the following pages; and this we are now the better enabled to do, as the late dreadful earthquake of the 1st of November 1755 supplies us with more * facts, and

* Many of these facts are collected together in the 49th volume of the Philosophical Transactions. The same are also to be found, with

and those better related, than any other earthquake of which we have an account.

2. That these concussions should owe their origin to something in the air, as it has sometimes been imagined, seems very ill to correspond with the phænomena. This, I apprehend, will sufficiently appear, as those phænomena are hereafter recounted; nor does there appear to be any such certain and regular connexion between earthquakes and the state of the air, when they happen, as is supposed by those who hold this opinion. It is said, for instance, that earthquakes always happen in calm still weather: but that this is not always so, may be seen in an account of the * earthquakes in Sicily of 1693, where we are told, “ the south winds have blown very much, which
“ still have been impetuous in the most sensible
“ earthquakes, and the like has happened at other
“ times.”

3. Other examples to the same purpose we have in an account of the earthquakes that happened in New England in 1727 and 1728; the author of

with some additional ones, in “ The History and Philosophy of “ Earthquakes,” (a work well worth the perusal of those, who are desirous of being acquainted with this subject). The author of it has given us, besides the aforesaid facts, a very judicious abridgment of ten of the most considerable writers upon the subject. I have taken the greatest part of my authorities either from this author, or the Philosophical Transactions, that those who would wish to examine them, may have an opportunity of doing it the more easily; some things only, which were not to be met with in these, and which yet were necessary to my purpose, I have been obliged to seek for elsewhere.

* See Philos. Trans. N° 207. or vol. ii. p. 408. Lowthorp’s Abridgment.

which

which says, that he could neither observe any connexion between the weather and the earthquakes, nor any prognostic of them; for that they happened alike in all kinds of weather, at all times of the tides, and at all times of the moon*.

4. If, however, it should still be supposed, notwithstanding these instances to the contrary, that there is some general connexion between earthquakes and the weather, at the time when they happen, yet, surely, it is far more probable, that the air should be affected by the causes of earthquakes, than that the earth should be affected in so extraordinary a manner, and to so great a depth; and that this,

* See *Philos. Transf.* N° 409. or vol. vi. part ii. p. 202. *Fames's Abridgment.*—To these authorities, we may add the opinion of *Mons. Bertrand*, who expresses himself, upon this occasion, in the following manner. “Aristotle, Pliny, and Seneca, tell us, that earthquakes are preceded by a calm and serene air. This is, indeed, often the case, but not always. I don’t know, upon an examination of the whole, if there are not as many exceptions to this rule, as examples that confirm it. Some authors again have thought, that they might look on a dark sky, lightnings, and sudden storms, as the forerunners of earthquakes.” Then relating some instances of shocks that happened in calm and serene weather, he adds, “On the other hand, it appears, from the examples, which we have before related, that many earthquakes have happened at the time of great rains, violent winds, and with a cloudy sky; so that one cannot find any certain prognostic of them in the state of the atmosphere.” See *Memoires Historiques et Physiques sur les tremblemens de Terre*, par *Aronf. Bertrand*, a la Haye 1757. This author, in these sensible memoirs, has obliged the public with a circumstantial account of all the facts he could collect, relating to the earthquakes of Switzerland, or those of other places, that seemed to be connected with them. The whole seems to be done with care and fidelity, and without the least attachment to any particular system.

and

and all the other circumstances attending these motions, should be owing to some cause residing in the air.

5. Let us then, rejecting this hypothesis, suppose, that earthquakes have their origin under ground, and we need not go far in search of a cause, whose real existence in nature we have certain evidence of, and which is capable of producing all the appearances of these extraordinary motions. The cause I mean is subterraneous fires. These fires, if a large quantity of water should be let out upon them suddenly, may produce a vapour, whose quantity and elastic force may be fully sufficient for that purpose. The principal facts, from which I would prove, that these fires are the real cause of earthquakes, are as follow.

SECTION I.

6. *First*, The same places are subject to returns of earthquakes, not only at small intervals for some time after any considerable one has happened, but also at greater intervals of some ages.

7. Both these facts sufficiently appear, from the accounts we have of earthquakes. The tremblings and shocks of the earth at * Jamaica in 1692, at * Sicily in 1693, and at * Lisbon in 1755, were repeated sometimes at larger, and sometimes at smaller intervals, for several months. The same thing has been observed in all other very violent earthquakes. At † Lima, from the 28th October 1746, to the

* See the accounts of these in the *Philos. Trans.*

† See Antonio d'Ulloa's *Voyage to Peru*, part ii. book i. ch. 7.

24th February 1747 (the time when the account of them was sent from thence), there had been numbered no less than 451 shocks, many of them little inferior to the first great one, which destroyed that city.

8. The returns of earthquakes also, in the same places, at larger distances of time, are confirmed by all history. Constantinople, and many parts of Asia Minor, have suffered by them, in many different ages: Sicily has been subject to them, as far back as the remains even of fabulous history can inform us of: Lisbon did not feel the effects of them for the first time in 1755: Jamaica has frequently been troubled with them, since the English first settled there; and the Spaniards, who were there before, used to build their houses of wood, and only one story high, for fear of them: * Lima, Callao, and the parts adjacent, were almost totally destroyed by them twice, within the compass of about sixty years, scarce any building being left standing, and the latter being both times overflowed by the sea: nor were these the only instances of the like kind, which have happened there; for, from the year 1582 to 1746, they have had no less than sixteen very violent earthquakes, besides an infinity of less considerable ones; and the Spaniards, at their first settling there, were told by the old inhabitants, when they saw them building high houses, that they were building their own sepulchres †.

9. Secondly,

* See the place above-quoted.

† What is here said, is taken from d'Ulloa's Voyage to Peru, the History and Philosophy of Earthquakes, the Philos. Trans. &c.
where

9. *Secondly*, Those places that are in the neighbourhood of burning mountains, are always subject to frequent earthquakes; and the eruptions of those mountains, when violent, are generally attended with them.

10. Asia Minor and Constantinople may be looked upon as in the neighbourhood of Santerini. The countries also about * *Ætna*, *Vesuvius*, mount *Hæcla*, &c. afford us sufficient proofs to the same purpose. But, of all the places in the known world, I suppose, no countries are so subject to earthquakes, as † *Peru*, *Chili*, and all the western parts of *South America*; nor is there any country in the known world so full of volcanos: for, throughout all that long range of mountains, known by the name of the *Andes*, from 45 degrees south latitude, to several degrees north of the line, as also throughout all *Mexico*, being about 5000 miles in extent, there is a continued chain of them ‡.

11. *Thirdly*, The motion of the earth in earthquakes is partly tremulous, and partly propagated by waves, which succeed one another sometimes at larger and sometimes at smaller

where many more examples, to the same purpose, are to be met with. See also *Memoires sur les tremblemens de Terre*; in which are mentioned above 130 repetitions of earthquakes, that have happened, within the compass of 960 years, in *Switzerland*.

* See many instances of this in vol. ii. of *Lowthorp's Abr. of the Philos. Transf.*

† *Monsr. Bouguer* says, that scarce a week passes without earthquakes in some part of *Peru*. See *Hist. of Earthq.* p. 205.

‡ See the Maps of these countries, *Condamine's Voyage down the Maranon*, *Acosta's Nat. Hist. of the Indies*, &c.

distances; and this latter motion is generally propagated much farther than the former.

12. The former part of this proposition wants no confirmation: for the proof of the latter, *viz.* the wave-like motion of the earth, we may appeal to many accounts of earthquakes: it was very remarkable in the two, which happened at Jamaica in * 1687-8 and * 1692. In an account of the former, it is said, that a gentleman there saw the ground rise like the sea in a wave, as the earthquake passed along, and that he could distinguish the effects of it, to some miles distance, by the motion of the tops of the trees on the hills. Again, in an account of the latter, it is said, “ the ground heaved and swelled, “ like a rolling swelling sea,” inasmuch, that people could hardly stand upon their legs by reason of it.

13. The same has been observed in the earthquakes of † New England, where it has been very remarkable. A gentleman giving an account of one, that happened there the 18th November 1755, says, the earth rose in a wave, which made the tops of the trees vibrate ten feet, and that he was forced to support himself, to avoid falling, whilst it was passing.

14. The same also was observed at ‡ Lisbon, in the earthquake of the 1st November 1755, as may be

* See Phil. Trans. N^o 209. or vol. ii. Lowthorp's Abridgment, p. 410.

† See Philos. Trans. vol. i. p. 1, &c.

‡ See the accounts collected together, in the 49th volume of the Philos. Trans. or in Hist. and Philos. of Earthq. and particularly p. 315. where it is said, “ A most dreadful earthquake shook by
“ short,

be plainly collected from many of the accounts that have been published concerning it, some of which affirm it expressly: and this wave-like motion was propagated to far greater distances than the other tremulous one, being perceived by the motion of waters, and the hanging branches in churches, through all Germany, amongst the Alps, in Denmark, Sweden, Norway, and all over the British isles.

15. *Fourthly*, It is observed in places, which are subject to frequent earthquakes, that they generally come to one and the same place, from the same point of the compass. I may add also, that the velocity, with which they proceed, (as far as one can collect it from the accounts of them) is the same; but the velocity of the earthquakes of different countries is very different.

16. Thus all the shocks, that succeeded the first great one at Lisbon in 1755, as well as the first itself, came from the * north-west. This is asserted by the person, who says, he was about writing a history of the earthquakes there: all the other accounts also confirm the same thing; for what some say, that they came from the north, and others, that they came

“ short, but quick vibrations, the foundations of all Lisbon;
 “ then, with a scarcely perceptible pause, the nature of the motion changed, and every building was tossed like a waggon
 “ driven violently over rough stones, which laid in ruins almost
 “ every house, church, &c.”

For the wave-like motion at Oporto, see Phil. Trans. vol. xlix. p. 418. for the same at Gibraltar, see Hist. and Philos. of Earthq. p. 322.

* See Philos. Trans. vol. xlix. p. 410.

from the west, cannot be looked on as any reasonable objection to this, but rather the contrary. The velocity also, with which they were all propagated, was the same, being at least equal to that of sound; for they all followed * immediately after the noise that preceded them, or rather the noise and the earthquake came together: and this velocity agrees very well with the intervals between the time when the first shock was felt at Lisbon, and the time when it was felt at other distant places, from the comparison of which, it seems to have travelled at the rate of more than † twenty miles *per* minute.

17. An historical account of the earthquakes, which have happened in ‡ New England, says, that, of five considerable ones, three are known to have come from the same point of the compass, *viz.* the north-west: it is uncertain from what point the other two came, but it is supposed that they came from the same with the former. The || velocity of these has been much less than that of the Lisbon earthquakes: this appears from the interval between the preceding noise, and the shock, as well as from the wave-like motion before-mentioned.

* See *Philos. Transf.* vol. xlix. p. 414. or *Hist. and Philos. of Earthq.* p. 315.

† See Art. 97.

‡ See *Philos. Transf.* vol. i. p. 9.

|| As in some earthquakes the velocity, with which they are propagated, is much less than in others, it is evident, that they can by no means be owing to any cause residing in the air: for any shock communicated to the air, must necessarily move with a velocity neither greater nor less than that of sounds; that is, at the rate of about thirteen miles *per* minute.

18. All the greater earthquakes, that have been felt at * Jamaica, seem, by the accounts given of them, to have come from the sea, and, passing by Port-Royal, to have gone northwards. The velocity of these also was far short of the velocity of the Lisbon earthquakes.

19. The earthquake of † London, on the 8th of March 1750, was supposed to move from east to west. I have been credibly informed, that the same thing happened in a flight shock, which was felt there in the last century, as the person, who told me this, had an opportunity of observing; for being, by accident, in a scalemaker's shop at the time when it happened, he found that all the scales vibrated from east to west.

20. All the shocks that have been lately felt at Brigue in Valais, have likewise come from the same point of the compass, viz. the south ‡.

21. *Fifthly*, The great Lisbon earthquake has been succeeded by several local ones since, the extent of which has been much-less.

22. Such were the earthquakes in Switzerland; those on the borders of France and Germany; those in Barbary, &c. ||

* See the accounts of them in Philos. Transf. N° 209. or vol. ii. Lowthorp's Abr. p. 410, &c.

† See Hist. and Philos. of Earthq. p. 250. or Philos. Transf. vol. x. Martyn's Abr. Meteorology, passim.

‡ See Philos. Transf. vol. xlix. p. 620. The same has been observed at Smyrna also, see Philos. Transf. N° 495, or Martyn's Abr. vol. x. p. 526.

|| See the accounts of these collected together in Philos. Transf. vol. xlix. or in the Hist. and Philos. of Earthq.

S E C T. II.

23. How well soever these facts may agree with the supposition before laid down, That subterraneous fires are the cause of earthquakes, one doubt, however, may perhaps remain; *viz.* how it is possible that fires should subsist, which have no communication with the outward air? In answer to this, I might alledge the example of green plants, which take fire by fermentation, when laid together in heaps; where the admission of the outward air is so far from being necessary, that it will effectually prevent their doing so. But, to pass by this, we have many instances more immediately to the purpose.

24. It can hardly be supposed, that the fires of the generality of volcanos receive any supply of fresh air (for this must effectually be prevented by that vapour, which is continually rushing out at all their vents), and yet they subsist, and frequently even increase, for many ages. Now, these are fires of the very same kind with those, which I suppose to be the cause of earthquakes. Other facts, still more expressly to the purpose, are as follow:

25. In the earthquake of the 1st of November 1755, we are told, that both smoke and light flames were seen on the coast of Portugal, near Colares; and that, upon occasion of some of the succeeding shocks, a slight smell of sulphur was perceived to accompany a "fog, which came from the sea, from the same quarter, whence the smoke appeared *."

* See Philos. Transf. vol. xlix. p. 414, &c.

26. In an account of an earthquake in New England, it is said, that at Newbury, forty miles from Boston, the earth opened, and threw up several cart-loads of sand and ashes; and that the sand was also slightly impregnated with sulphur, emitting a blue flame, when laid on burning coals *.

27. One of the relaters of the earthquake in Jamaica in 1692, has these words: “ In Port-Royal, “ and in many places all over the island, much sulphureous combustible matter hath been found “ (supposed to have been thrown out upon the “ opening of the earth), which, upon the first touch “ of fire, would flame and burn like a candle.

28. “ St. Christopher’s was heretofore much “ troubled with earthquakes, which, upon the eruption there of a great mountain of combustible matter, which still continues, wholly ceased, and have “ never been felt there since †.”

29. Again, we are told, that, on the 20th November 1720, a burning ‡ island was raised out of the sea, near Tercera, one of the Azores, at which place, several houses were shaken down by an earthquake, which attended the eruption of it. This island was about three leagues in diameter, and nearly round; from whence it is manifest, that the quantity of pumice stones and melted matter, which must have been requisite to form it, was amazingly great:

* See Philos. Transf. N° 409. or vol. vi. part ii. p. 201. Eames’s Abr.

† See Philos. Transf. N° 209. or vol. ii. p. 418. Lowthorp’s Abr.

‡ See Philos. Transf. N° 372. or vol. vi. part ii. p. 203. Eames’s Abr.

in all probability, it must have far exceeded all that has been thrown out of *Ætna* and *Vesuvius* together within the last two thousand years. This may serve to satisfy us, that the fire which occasioned all this, must have subsisted for many years, not to say ages, and this without any communication with the external air. It is worth observing, that * several instances of this kind have happened amongst the *Azores*. There are besides many marks of subterraneous fires about these islands, several places sending up smoke or flames. These islands are also subject to violent and frequent earthquakes.

30. We have more instances to the same purpose, near the island of *Santerini* in the *Archipelago*, where there have been several little islands raised out of the sea by a submarine volcano. The eruption of one of these in the year 1708, with all the circumstances that attended it, we have a very good account of in the † *Philosophical Transactions*. It was raised in a place where the sea had been formerly 100 fathoms deep, and was attended with earthquakes before it shewed itself above water, as well as after. It is reported, that the island of *Santerini* itself was originally raised out of the sea in the same manner; but, be that as it will, we have certain accounts of new islands raised there, or additions made to the old ones, from time to time, for above 1900 years backwards, and there have always been earthquakes at the time of these eruptions.

* See *Hist. and Philos. of Earthquakes*, under the titles *Azores*, *Islands raised*, &c.

† See N^o 314, 317, and 332. or vol. v. part ii. p. 196. *Jones's Abr.*

31. Another example of the same kind happened at * Manila, one of the Philippine islands, in the year 1750. This also was attended with violent earthquakes, to which that island, as well as the rest of the Philippines, is very much subject.

32. We may add to these, the many instances of vast quantities of † pumice stones, which have been sometimes found floating upon the sea, at so great a distance from the shore, as well as from any known volcano, that there can be little doubt of their being thrown up by fires subsisting under the bottom of the ocean.

33. From these instances, we may, with great probability, conclude, that the fires of volcanos produce earthquakes: I do not, however, suppose, that the earthquakes, which are frequently felt in the neighbourhood of volcanos, are owing to the fires of those volcanos themselves; for volcanos, giving passage to the vapours that are there formed, should rather prevent them, as in the instance at St. Christopher's, before-mentioned.

34. We also meet with frequent instances confirming the same thing amongst the Andes. Antonio d'Ulloa (speaking of what happens amongst these mountains) says, " Experience shews us, that, upon
" the fresh breaking out of any volcano, it occasions
" so violent a shock to the earth, that all the villages,
" which are near it, are overthrown and destroyed,

* See Philos. Transf. vol. xlix. p. 459.

† See Philos. Transf. N^o 372. or vol. vi. part ii. p. 204. and N^o 402. or vol. vii. part ii. p. 43. Eames's Abr.

“ as it happened in the case of the mountain * Car-
 “ guayrafo. This shock, which we may, without
 “ the least impropriety, call an earthquake, is sel-
 “ dom found to accompany the eruptions, after an
 “ opening is once made ; or, if some small trembling
 “ is perceived, it is very inconsiderable ; so that,
 “ after the volcano has once found a vent, the shocks
 “ cease, notwithstanding the matter of it continues
 “ to be on fire.” The greater earthquakes, there-
 fore, seem rather to be occasioned by other fires, that
 lie deeper in the same tract of country ; and the erup-
 tions of volcanos, which happen at the same time
 with earthquakes, may, with more probability, be
 ascribed to those earthquakes, than the earthquakes
 to the eruptions, whenever, at least, the earthquakes
 are of any considerable extent. If this don’t appear
 sufficiently manifest at present, it will, perhaps, be
 better understood, by applying to the present pur-
 pose, what will be said hereafter concerning local
 earthquakes.

* It does not appear altogether certain, from the expression made use of in the French translation (from whence I have taken this), that Carguayrafo might not have been a volcano in former times, which is asserted to have been the case by Mons. Condamine. It is possible also, that the same may be true of those four mentioned in the next article ; and, indeed, it is difficult to know it to be otherwise, in any instance, among the Andes, where the volcanos are generally found at inaccessible heights. But allowing, that all these were only old volcanos, which broke out afresh, yet they will serve at least to swell the number of them in the same neighbourhood, as well as to shew us, that there may, very probably, be many more, which lie hid : for these shewed no marks of their existence, till, by their eruption, they melted a vast quantity of snow, with which they were before covered, and which, being reduced to water, did great damage, by overflowing the country round about.

S E C T. III.

35. It may be asked, perhaps, why we should suppose, that several subterraneous fires exist in the neighbourhood of volcanos? In evidence of this, we have frequent instances of new volcanos breaking out in the neighbourhood of old ones: Carguayrafo, just mentioned, may supply us with one example to this purpose; and, in the night of the 28th of October 1746, in which Lima and Callao were destroyed, no less than four * new ones burst forth in the adjacent mountains.

36. To the same purpose, we may allege the instances of many volcanos lying together in the same tract of country: as for example, the many places, “not so few as forty,” amongst the Azores, which either do now or have formerly sent forth smoke and flames; the many volcanos also amongst the Andes, already mentioned: thus *Ætna*, *Strombolo*, and *Vesuvius*, I may add *Solfatara* too, are all in the same neighbourhood: and *Monf. Condamine* says, he has traced † lavas, exactly like those of *Vesuvius*, all the way from *Florence* to *Naples*. In ‡ *Iceland* also, we have, besides *Hæcla*, not only several other volcanos, but also a great number of places, that send up sul-

* See d’Ulloa’s *Voyage to Peru*, part ii. book i. chap. 7.

† See *Phil. Transf.* vol. xlix. p. 624. All these lavas, as well as the volcanos just mentioned, lie in a continued line. The same thing holds good in the volcanos of the Andes also. This is a fact I must desire the reader to attend to, as it serves to confirm a very material doctrine, which I shall have occasion to mention hereafter. See art. 44, 45, and 46.

‡ See *Horrebow’s Natural History of Iceland*.

phureous vapours. But the examples of this kind are so frequent, that there are few instances to be produced of single volcanos, without evident marks, either that there have been others formerly in their neighbourhood, or that there are, at present, subterraneous fires near them.

37. This frequency of subterraneous fires, in the neighbourhood of volcanos, will appear still more probable, if we consider the internal structure of the earth; and, as it will be necessary also, in order to understand what follows, to know a little more of this matter, than what falls under common observation, I shall endeavour to give the reader some account of it.

38. The earth then (as far as one can judge from the appearances), is not composed of heaps of matter casually thrown together, but of regular and uniform strata. These strata, though they frequently do not exceed a few feet, or perhaps a few inches, in thickness, yet often extend in length and breadth for many miles, and this without varying their thickness considerably. The same stratum also preserves a uniform character throughout, though the strata immediately next to each other are very often totally different. Thus, for instance, we shall have, perhaps, a stratum of potters clay; above that, a stratum of coal; then another stratum of some other kind of clay; next, a sharp grit sand stone; then clay again; next, perhaps, sand stone again; and coal again above that; and it frequently happens, that none of these exceed a few yards in thickness. There are, however, many instances, in which the same kind of matter is extended to the depth of some
hundreds

hundreds of yards ; but in all these, a very few only excepted, the whole of each is not one continued mass, but is again subdivided into a great number of thin laminæ, that seldom are more than one, two, or three feet thick, and frequently not so much.

39. Beside the horizontal division of the earth into strata, these strata are again divided and shattered by many perpendicular fissures, which are in some places few and narrow, but oftentimes many, and of considerable width. There are also many instances, where a particular stratum shall have almost no fissures at all, though the strata both above and below it are considerably broken : this happens frequently in clay, probably on account of the softness of it, which may have made it yield to the pressure of the superincumbent matter, and fill up those fissures which it originally had ; for we sometimes meet with instances in mines, where the correspondent fissures in an upper and lower stratum are interrupted in an intermediate stratum composed of clay, or some such soft matter.

40. Though these fissures do sometimes correspond to one another in the upper and lower strata, yet this is not generally the case, at least not to any great distance : those clefts, however, in which the larger veins of the ores of metals are found, are an exception to this observation ; for they sometimes pass through many strata, and those of different kinds, to unknown depths.

41. From this constitution of the earth, *viz.* the want of correspondence in the fissures of the upper and lower strata, as well as on account of those strata which are little or not at all shattered, it will come to pass,

pass, that the earth cannot easily be separated in a direction * perpendicular to the horizon, if we take any considerable portion of it together; but in the horizontal direction, as there is little or no adhesion between one stratum and another, it may be separated without difficulty.

42. Those fissures which are at some depth below the surface of the earth, are generally found full of water; but all those that are below the level of the sea, must always be so, either from the oozing of the sea, or rather of the land waters between the strata.

43. The strata of the earth are frequently very much bent, being raised in some places, and depressed in others, and this sometimes with a very quick ascent or descent; but as these ascents and descents, in a great measure, compensate one another, if we take a large extent of country together, we may look upon the whole set of strata, as lying nearly horizontally. What is very remarkable, however, in their situation, is, that from most, if not all, large tracts of high and mountainous countries, the strata lie in a situation more inclined to the horizon, than the country itself, the † mountainous countries being generally,

* What I said before of those deep clefts, in which metals are found, will not affect this conclusion; for they are considerably different from either perpendicular or plane sections of earth; they are frequently interrupted by strata of clay, or other soft matter; and they are, in most parts, either filled up with rubbish, or with ores and spars, that adhere as firmly to the rocks on both sides, as if they composed one continued stratum with them.

† It seems very probable, from many appearances, not only that the mountainous countries are formed out of the lower strata of the earth, but that sometimes the highest hills in them are formed out



Fig. 1.

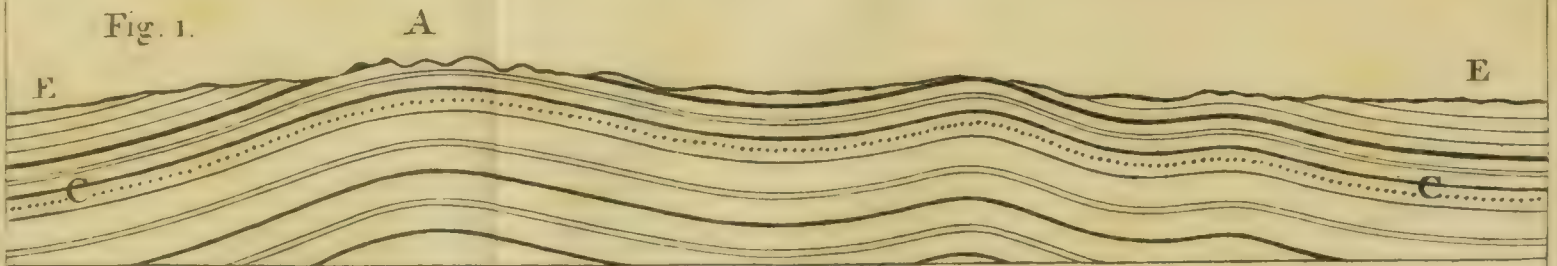


Fig. 3.

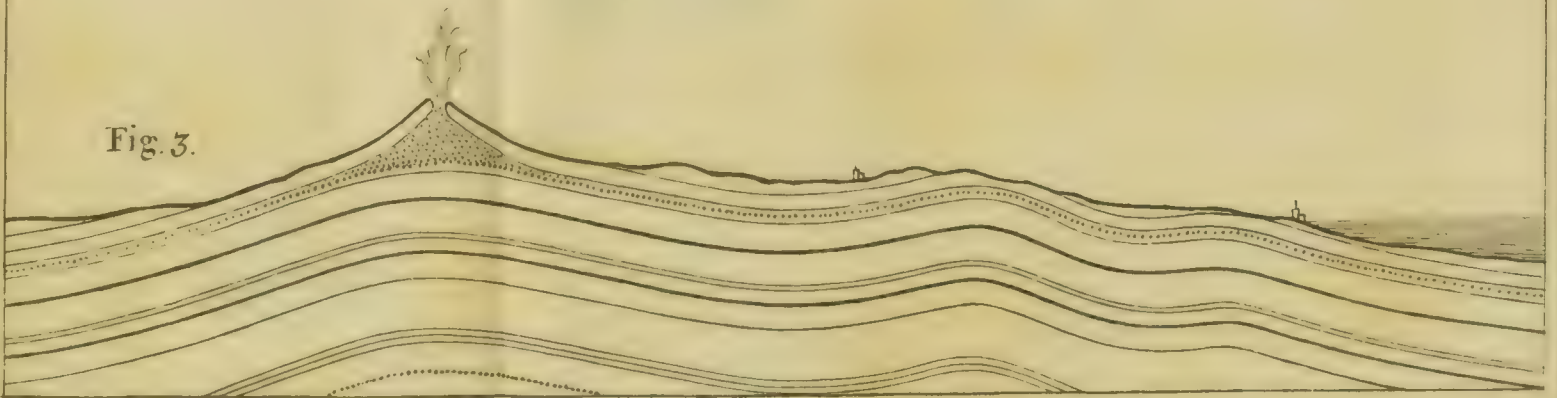


Fig. 4.

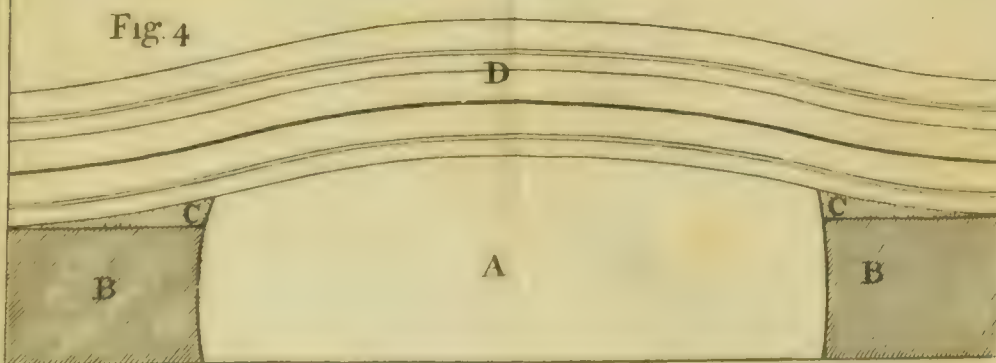
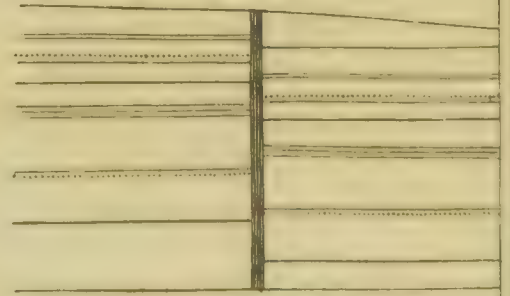


Fig. 2.



generally, if not always, formed out of the lower strata of earth. This situation of the strata may be not unaptly represented in the following manner. Let a number of leaves of paper, of several different sorts or colours, be pasted upon one another; then bending them up together into a ridge in the middle, conceive them to be reduced again to a level surface, by a plane so passing through them, as to cut off all the part that had been raised; let the middle now be again raised a little, and this will be a good general representation of most, if not of all, large tracts of mountainous countries, together with the parts adjacent, throughout the whole world *.

44. From this formation of the earth, it will follow, that we ought to meet with the same kinds of earths, stones, and minerals, appearing at the surface, in long narrow slips, and lying parallel to the greatest rise of any long ridges of mountains; and so, in fact, we find them. The Andes in South America, as it has been said before, have a chain of volcanos, that extend in length above 5000 miles: these volcanos, in all probability, are all derived from the † same

out of strata still lower than the rest, which, perhaps, may always be the case, where they have volcanos in them. [See a representation of this in Tab. XIII. Fig. 3.] In other instances, however, it often happens, that the hills, to which these high lands serve as a base, are not only formed out of the strata next above them, but they stand, as it were, in a dish, as if they had depressed the ground, on which they rest, by their weight.

* Fig. 1. represents a section of a sett of strata, lying in the situation just described: the section is supposed to be made at right angles to the length of the ridge, and perpendicular to the horizon.

† See the notes to art. 36 and 53. See also Fig. 3.

stratum.

stratum. Parallel to the Andes, is the Sierra, another long ridge of mountains, that run between the Andes and the sea; and “these two ridges of mountains run “ within sight of one another, and almost equally, “ for above a thousand leagues together *,” being each, at a medium, about twenty leagues wide. The gold and silver mines wrought by the Spaniards, are found in a tract of country parallel to the direction of these, and extending through a great part of the length of them.

45. The same thing is found to obtain in North America also. The great lakes, which give rise to the river St. Laurence, are kept up by a long ridge of mountains, that run nearly parallel to the eastern coast. In descending from these towards the sea, the same sets † of strata, and in the same order, are generally met with throughout the greatest part of their length.

46. In Great Britain, we have another instance to the same purpose, where the direction of the ridge varies about a point from due north and south, lying nearly from ‡ N. by E. to S. by W. There are many more instances of this to be met with in the world, if we may judge from circumstances, which make it highly probable, that it obtains in a great number of places, and in several they seem to put it almost out of doubt.

47. The reader is not to suppose, however, that, in any instances, the highest rise of the ridge, and

* See Acoſta's Natural Hiſtory of the Indies.

† See Lewis Evans's Map and Account of North America.

‡ Of this I could give many undoubted proofs, if it would not too far exceed the limits of my preſent deſign, and which, for that reaſon, I am obliged to omit.

the inclination of the strata from thence to the countries on each side, is perfectly uniform; for they have frequently very considerable inequalities, and these inequalities are sometimes so great, that the strata are bent for some small distance, even the contrary way from the general inclination of them. This often makes it difficult to trace the appearance I have been relating, which, without a general knowledge of the fossil bodies of a large tract of country, it is hardly possible to do.

48. At considerable distances from large ridges of mountains, the strata, for the most part, assume a situation nearly level; and as the mountainous countries are generally formed out of the lower strata, so the more level countries are generally formed out of the upper strata of the earth.

49. Hence it comes to pass, that, in countries of this kind, the same strata are found to extend themselves a great way, as well in breadth as in length: we have an instance of this in the chalky and flinty countries of England and France, which (excepting the interruption of the Channel, and the clays, sands, &c. of a few counties) compose a tract of about three hundred miles each way.

50. Besides the raising of the strata in a ridge, there is another very remarkable appearance in the structure of the earth, though a very common one; and this is what is usually called by miners, the trapping down of the strata; that is, the whole set of strata on one side a cleft are sunk down below the level of the corresponding strata on the other side. If, in some cases, this difference in the level of the strata, on the different sides of the cleft, should be

very considerable, it may have a great effect in producing some of the singularities of particular earthquakes *.

P A R T II.

51. **I**N the former part of this essay, I have recounted some of the principal appearances of earthquakes, as well as those particulars in the structure of the earth, upon which I suppose these appearances to depend. From what has been already said, I think it is sufficiently manifest, that, in some instances at least, earthquakes are actually produced by subterraneous fires; it now, therefore, remains to be shewn, how all the appearances above-recited, as well as many other minuter circumstances attending earthquakes, may be accounted for from the same cause.

S E C T. I.

52. The returns of earthquakes in the same places, either at small or large intervals of time, are very consistent with the cause assigned: subterraneous fires, from their analogy to volcanos, might reasonably be supposed to subsist for many ages, though we had not those instances † already mentioned, which put the

* Fig. 2. represents a section of the strata trapping down after the manner just described. The section is supposed to be made perpendicularly to the horizon, and at right angles to the direction of the cleft: an instance of this kind, amongst the coal miners of Mendip in Somersetshire, is mentioned in the *Philos. Trans.* See the account of it, together with a drawing, in N^o 360. or Jones's *Abr.* vol. iv. part ii. p. 260.

† See art. 28 to 32 inclusive.

matter out of doubt. And, as it frequently happens, that volcanos rage for a time, and then are quiet again for a number of years; so we see earthquakes also frequently repeated for some small time, and then ceasing again for a long term, excepting, perhaps, now and then some slight shock. And this analogy between earthquakes, and the effects of volcanos, is so great, that I think it cannot but appear striking to any one, who will read the accounts of both, and compare them together. The raging of volcanos is not one continued and uniform effect; but an effect, that is repeated at unequal intervals, and with unequal degrees of force: thus, for instance, we shall have, perhaps, two or three blasts discharged from a volcano, succeeding one another at the interval of a few seconds only: sometimes the intervals are of a quarter of an hour, an hour, a day, or perhaps several days. And as these intervals are very unequal, so is the violence of the blasts also: sometimes stones, &c. are thrown, by these blasts, to the distance of some miles; at other times, perhaps, not to the distance of a hundred yards. The same difference is observed in the intervals and violence of the shocks of earthquakes, which are repeated at small intervals for some time.

S E C T. II.

53. The great frequency of earthquakes in the neighbourhood of burning mountains, is a strong argument of their proceeding from a cause of the same kind: and the analogy of several volcanos lying together in the same tract of country, as well as new ones breaking out in the neighbourhood of old ones,

tends greatly to confirm this opinion; but what makes it still the more probable, is that peculiarity in the structure of the earth, already mentioned. I observed before, that the same strata are generally very extensive, and that they commonly lie more inclining from the mountainous countries, than the countries themselves: these circumstances make it very probable, that those * strata of combustible materials, which break

* I. has been imagined by some authors, that volcanos are produced by the pyrites of veins, and that they do not owe their origin to the matter of strata. In order to prove this, it is alleged, that volcanos are generally found on the tops of mountains, and that those are the places in which veins of pyrites are generally lodged. This argument being taken from observations that have their foundation in nature, ought not to go unanswered. In the first place, then, the pyrites of veins, or fissures, are not found in sufficient quantities, or extending to a sufficient breadth, to be supposed capable of producing the fires of volcanos: it very rarely happens, that we meet with a vein or fissure five or six yards wide; and when we meet with such an one, yet, perhaps, not a twentieth part of it at most shall be filled with pyrites; but the fires of volcanos, instead of being long and narrow, as if the matter that supplied them was deposited in veins, are generally round, and of far greater breadth than veins can be supposed to be. *Monf. Bouguer* says, that the mouth of the volcano *Cotopaxi* is, at this time, five or six hundred fathoms wide; [see *Hist. and Phil. of Earthquakes*, p. 195.] and the burning island that was raised out of the sea near *Tercera*, as before-mentioned, was almost three leagues in diameter, and nearly round. [See art: 29.]

Besides this, it is very difficult to conceive how any matters lodged in veins can ever take fire; for, excepting where the veins are extremely narrow, they are almost always drowned in a very great quantity of water, which has free access to every part of them: neither are the pyrites of veins, by any means, so apt to take fire of themselves, as those of strata; and if, indeed, there are any of them that will do so, yet they are but few in comparison of those which will not: all those, which, beside iron and sulphur, contain copper, or arsenic, even in a very small proportion, are not

at

Break out in volcanos on the tops of the hills, are to be found at a considerable depth under ground in the level and low countries near them. If this should be the case,

at all subject to inflame of themselves. On the other hand, most of the pyrites of strata, if not all of them, have this property more or less. There are also two sorts of strata, in which pyrites are lodged in the greatest abundance, that have the same property, and that frequently in as great a degree as themselves: these are coals and aluminous earths, or shale. There are some kinds of both these, that, upon being exposed to the external air for a few months, will take fire of themselves, and burn. These two sorts of strata are also near akin to each other; they are generally found to accompany each other; they are both of them generally intermixed with, or accompanied by strata of iron ore; and they both of them, for the most part, either contain, or are lodged amongst, the remains of vegetable bodies; and these remains of vegetable bodies, in the aluminous earths, are frequently either wholly, or in part, converted into pyrites, or coal, or both. Numberless, instances of this are to be met with in the aluminous shale of Whitby and other places.

It is very probable, that to some stratum of this kind the fires of volcanos are owing; and this seems to be confirmed by the similarity of the materials, which are thrown up or sublimated by the fires of volcanos, to the matter of the aluminous earths. Solfatara produces sulphur, alum, and sal ammoniac. The two former of these are very easily to be obtained from the aluminous earths, and, I suppose, the latter also; at least it is procurable from the foot of common fossil coals, and probably, therefore, from the foot of that coaly matter which is intermixed with such earths.

The aluminous earths, moreover, not only have several strata of iron ore lying in them, but they also contain a considerable proportion of iron in their composition. In correspondence to this, we find the lavas of volcanos, and other matters thrown out from thence, frequently containing a great deal of iron, the small dust of them readily adhering to the magnet.

As to the pyrites of veins, I much doubt whether they ever contain alum, or sal ammoniac; at least they are very rarely found to contain either the one or the other.

and:

and if the same * strata should be on fire in any places under such countries, as well as on the tops of the hills, all vapours, of whatsoever kind, raised from these fires, must be pent up, unless so far as they can open themselves a passage between the strata; whereas the vapours raised from volcanos find a vent, and are discharged in blasts from the mouths of them. Now, if, when they find such a vent, they are yet capable of shaking the country to the distance of ten or twenty miles round, what may we not expect from them, when they are confined? We may form some idea of the force and quantity of these vapours from their effects: it is no uncommon thing to see them throw up, at once, such clouds of sand, ashes, and pumice stones, as are capable of darkening the whole air, and covering the neighbouring country with a shower of dust, &c. to some miles distance: great stones also, of some tons weight, are often thrown to the distance of two or three miles by these explosions: and Mons. Bouguer tells us, that he met with stones

* It may be asked, perhaps, why a stratum liable to take fire in some places, should not take fire throughout the whole extent of it? In answer to this, it may be said, that the same stratum may differ a little in the richness of its combustible principles in different places; or, perhaps, the frequency of the fissures, either in the combustible stratum itself, or the stratum next to it, may let in so much water, as to prevent its taking fire, excepting in a few places; but, if this once happens, the fire will not easily be put out again, but it will spread itself, notwithstanding the fissures that lie in its way, though they are filled with water; for the matter on fire will be, in some degree at least, in a fluid state; and, for this reason, it must necessarily expel the water from the fissures, both on account of the extension of its own dimensions by the heat, and of the weight of the superincumbent earth, which, pressing it, will make it spread laterally.

in South America, of eight or nine feet diameter, that had been thrown from the volcano Cotopaxi, by one of these blasts, to the distance of more than * three leagues.

54. If we suppose that these vapours, when pent up, are the cause of earthquakes, we must naturally expect, from what has been just said, that the most extensive earthquakes should take their rise from the level and low countries; but more especially from the sea, which is nothing else than waters covering such countries. Accordingly we find, that the great earthquake of the 1st November 1755, which was felt at places near three thousand miles distant from each other, took its rise from under the sea; this is manifest, from that wave which accompanied it, as shall be shewn hereafter. The same thing is to be understood of the earthquake that destroyed Lima in the year 1746, which, it has been said, was felt as far as Jamaica; and, as it was more violent than the Lisbon earthquake, so, if this be true, it must, in all probability, have been more extensive also. There have been many other very extensive earthquakes in South America: Acosta says, that they have been often known to extend themselves one, two, or three hundred, and some even five hundred leagues, along the coast. These have been generally, if not always, attended with waves from the sea; but any minuter

* See Hist. and Philos. of Earthq. p. 195. Don Antonio d'Ulloa, an author of great veracity, speaking of the same thing, says, that "the whole plain [near Latacunga] is full of large pieces of rocks, "some of them thrown from the volcano Cotopaxi, by one of its "eruptions, to the distance of five leagues." See his Voyage to Peru, part i. book vi. chap. I.

circumstances accompanying them are not related. Indeed it is hardly to be expected that they should be observed, much less that they should be related, when they happened in a country so thinly inhabited, and where one may reasonably suppose, that, in general, only the grosser and more violent effects would be taken notice of.

S E C T. III.

55. I have said before, that I imagined earthquakes were caused by vapours raised from waters suddenly let out upon subterraneous fires. It is not easy to find any other cause capable of producing such sudden and violent effects, or of raising such an amazing quantity of vapour in so small a time. That the blasts, discharged from volcanos, are always produced from this cause, is highly probable; that they are often so, cannot admit of the least doubt. There can be no doubt, that considerable quantities of water must be often let out upon the fires of these volcanos, and whenever this happens, it will be immediately raised by the heat of them into a vapour, whose elastic force is capable of producing the most violent effects *.

56. Both

* There are many effects produced by the vapour of water, when intensely heated, which make it probable, that the force of gunpowder is not near equal to it. The effects of an exceeding small quantity of water, upon which melted metals are accidentally poured, are such, as, I think, could in no wise be expected from the like quantity of gunpowder. Founders, if they are not careful, often experience these effects to their cost. An accident of this kind happened about forty years since, at the casting of two brass cannon at Windmill-hill, Moorfields. “ The
“ heat

56. Both the tremulous and wave-like motion observed in earthquakes, may be very well accounted for

“ heat of the metal of the first gun drove so much damp into the
 “ mould of the second, which was near it, that as soon as the
 “ metal was let into it, it blew up with the greatest violence, tearing
 “ ing up the ground some feet deep, breaking down the furnace,
 “ untiling the house, killing many spectators on the spot, with the
 “ streams of melted metal, and scalding many others in a most miserable
 “ ferable manner.” [See the note at the end of process 44th of the
 English translation of Cramer’s Art of assaying Metals.]

Other instances of the violence of vapours raised from water, are frequently to be met with: one of Papin’s digesters being placed between the bars of a grate, where there was a fire, was, after some time, burst by the violence of the steam, the fire was all blown out of the grate, and a piece of the digester was driven against the leaf of a strong oak table, which it broke to pieces. [See Philos. Transf. N° 454. or Martyn’s Abr. vol. viii. p. 465.] The marquis of Worcester also, in his Century of Inventions, tells us, that he burst a cannon by the same means.

It has been sometimes imagined, that the vapours, which occasion earthquakes, were of the same kind with those fulminating damps, of which we often meet with instances in coal mines. Now, there are several things which make it very probable, that this is not the case: it is true, the force of such vapours is very great; we have had instances, where large beams of timber have been thrown to the distance of an hundred yards by them: [see Philos. Transf. N° 136. or vol. ii. p. 381. Lowthorp’s Abr.] but what is this to the force of that vapour, which could throw stones of twenty or thirty ton weight to the distance of three leagues? Nor, indeed, is it at all probable, that any vapour, already in the form of a vapour, can, by suddenly taking fire, increase its dimensions so much, as to produce that immense quantity of motion, which we observe in some earthquakes: but this is rather to be expected from some solid body, such as water, which is capable of being converted, and that almost instantly, into one of the lightest, and perhaps one of the most elastic, vapours in the world. Air, when heated to the greatest degree that it is capable of receiving from the hottest fires we can make, acquires a degree of elasticity about five times as great as that of common air: the vapour of

for from such a vapour. In order to trace a little more particularly the manner in which these two motions

gunpowder, whilst it is inflamed, has also about five times the elastic force which it has when cold. [See Robins's excellent tract on Gunnery.] Now, if we suppose a fulminating damp, of any kind, to increase its elasticity, when inflamed in the same proportion, this will be abundantly sufficient to make it produce any effects, which we have ever seen produced by any of the damps of mines, &c. And, indeed, whoever carefully examines the effects, either of the damps of mines, or of those fulminating damps, that are raised from some metals, when in fusion, or when they are dissolving in acids, will rather be inclined to think, that the force of inflamed vapours is so far from exceeding the proportion of five to one, that it falls considerably short of it.

But though we should suppose that this proportion holds good, where shall we find a place capable of containing a sufficient quantity of such a vapour, to produce the great effects of earthquakes? It will be said, perhaps, in subterraneous caverns. To this we may answer, that he, who is but moderately acquainted with the structure of the earth, and the materials of which it is composed, will be little inclined to allow of any great or extensive caverns in it. But, though this should be admitted, how can it come to pass that these caverns should not be filled with water? If it is alleged, that the water is expelled, as the vapour is formed, why should not the vapour, as it is supposed to be the lighter, be expelled, rather than the water, by the same passages by which the water is to be expelled? But let us suppose this difficulty also to be got over, and the water to be removed, and we shall then have a gage for the density of the vapour; for it must be just sufficient to make it capable of sustaining a column of water, whose height is equal to that of the surface of the sea above the bottom of the cavern, in which the vapour is supposed to be contained. Now, since the mean weight of earth, stones, &c. is not less than two and a half times the weight of water, this vapour must be increased to two and half times its original elasticity, before it can, in any wise, raise the earth above it; and if we suppose it to be increased to five times its original elasticity, it will then be no more than twice able to do so; in which case, so much vapour only can be discharged from the cavern, to produce an earthquake, as is equal to the
content

motions will be brought about, let us suppose the roof over some subterraneous fire to fall in. If this should be the case, the earth, stones, &c. of which it was composed, would immediately sink in the melted matter of the fire below: hence all the water contained in the fissures and cavities of the part falling in, would come in contact with the fire, and be almost instantly raised into vapour. From the first effort of this vapour, a cavity would be formed (between the melted matter and superincumbent earth) filled with vapour only, before any motion would be perceived at the surface of the earth: this must necessarily happen, on account of the * compressibility of

content of the cavern: and what must the size of that cavern be, which could contain vapour enough to produce the earthquake of the 1st of November 1755, in which an extent of earth of near three thousand miles diameter was considerably moved? or how can we suppose, that the roof of such a cavern, when so violently shaken, should avoid falling in? especially, as it is hardly to be supposed, that any inflamed vapour whatsoever should be able to move the earth over these caverns, if they lay at any great depth, since the weight of less than three miles depth of earth is capable of retaining the inflamed vapour of gunpowder within the original dimensions of the gunpowder itself; and common air, compressed by the same weight (supposing the known law of its compression to hold so far), would be of greater density than water.

We may ask still farther, whence such vast quantities of vapour should be formed, or what sources they must be, which would not be exhausted (if they were not again replenished) by a very few repetitions of such immense discharges.

* The compressibility and elasticity of the earth, are qualities which don't show themselves in any great degree in common instances, and therefore are not commonly attended to. On this account it is, that few people are aware of the great extent of them, or the effects that may arise from them, where exceeding large quantities of matter are concerned, and where the compress-

of all kinds of earth, stones, &c. but as the compression of the materials immediately over the cavity, would

five force is immensely great. The compressibility and elasticity of the earth may be collected, in some measure, from the vibration of the walls of houses, occasioned by the passing of carriages in the streets next to them. Another instance to the same purpose, may be taken from the vibrations of steeples, occasioned by the ringing of bells, or by gusts of wind: not only spires are moved very considerably by this means, but even strong towers will, sometimes, be made to vibrate several inches, without any disjoining of the mortar, or rubbing of the stones against one another. Now, it is manifest, that this could not happen, without a considerable degree of compressibility and elasticity in the materials, of which they are composed: and if such small things as the weight of steeples, and the motion of bells in them, or a gust of wind, are capable of producing such effects, what may we not expect from the weight of great depths of earth? There are some circumstances, which seem to make it not altogether improbable, that the form and internal structure of the earth depend, in a great measure, upon the compressibility and elasticity of it. There are several things that seem to argue a considerably greater density in the internal, than the external part of the earth; and why may not this greater density be owing to the compression of the internal parts arising from the weight of the superincumbent matter, since it is probable, that the matter, of which the earth is composed, is pretty much of the same kind throughout? There is a still stronger argument for the earth's owing its form, in some measure, to the same cause; for it is found to be higher [see the French accounts of the measures of a degree of the meridian in France, Sweden, and America] at the æquator, than at the poles, in a greater proportion than it would be on account of the centrifugal force, if it was of uniform density; but, if we suppose the earth to be of less density in an æquatorial diameter than in the axis, the whole will then be easily accounted for, from the rising of the earth a little by its elasticity, the weight being in part taken off by the diurnal rotation: and that the earth is really a little denser in the axis, than in the æquatorial diameter, seems highly probable, from the experiments of pendulums compared with astronomical observations; for the forms of the earth derived from these, cannot be reconciled

would be more than sufficient to make them bear the weight of the superincumbent matter, this compression must be propagated on account of the elasticity of the earth, in the same manner as a pulse is propagated through the air; and again the materials immediately over the cavity, restoring themselves beyond their natural bounds, a dilatation will succeed to the compression; and these two following each other alternately, for some time, a vibratory motion will be produced at the surface of the earth. If these alternate dilatations and compressions should succeed one another at very small intervals, they would excite a like motion in the air, and thereby occasion a considerable noise. The noise that is usually observed to precede or accompany earthquakes, is probably owing partly to this cause, and partly to the grating of the parts of the earth together, occasioned by that wave-like motion before-mentioned.

57. After the water, that first came in contact with the fire, has formed a cavity, all the rest of the water contained in the fissures, immediately communicating with the hollow left by the part that fell in,

reconciled with each other, but upon this supposition. [See Mac-laurin's Fluxions, art. 681, &c.] It appears, from some late and accurate observations, that the æquatorial parts of the planet Jupiter also, as well as those of the earth, are a little higher than they would be, if their rise was owing to the centrifugal force, and he was of uniform density; but if we suppose him to be of less density in the æquatorial, than the polar regions, then the form may be such as he would assume from the respective gravitation of the several parts; and any fluid like our ocean, would not overflow the polar parts, (which, upon any other supposition, it must necessarily do) but would follow his general form, as our ocean does that of the earth.

must run out upon the fire, the steam taking its place. From hence may be generated a vast quantity of vapour, the effects of which shall be considered presently. This steam will continue to be generated, supposing the fire to be sufficiently great, till the fissures before-mentioned are evacuated, or till the water begins to flow very slowly; when the steam already formed will be removed by the elasticity of the earth, which will again subside, and, pressing upon the surface of the melted matter, will force it up a little way into all the clefts, by which the water might continue to flow out. By this means, all communication between the fire and the water will be prevented, excepting at these clefts, where the water, dripping slowly upon the melted matter, will gradually form a crust upon it, that will soon stop all farther communication in these places likewise; and the fissures, that had been before evacuated, will be again gradually replenished by the oozing of the water between the strata.

58. As a small quantity of vapour almost instantly generated at some considerable depth below the surface of the earth, will produce a vibratory motion, so a very large quantity (whether it be generated almost instantly, or in any small portion of time) will produce a wave-like motion. The manner in which this wave-like motion will be propagated, may, in some measure, be represented by the following experiment. Suppose a large cloth, or carpet, (spread upon a floor) to be raised at one edge, and then suddenly brought down again to the floor, the air under it, being by this means propelled, will pass along, till it escapes at the opposite side, raising the
cloth

cloth in a wave all the way as it goes. In like manner, a large quantity of vapour may be conceived to raise the earth in a wave, as it passes along between the strata, which it may easily separate in an horizontal direction, there being, as I have said before, little or no cohesion between one stratum and another. The part of the earth that is first raised, being bent from its natural form, will endeavour to restore itself by its elasticity, and the parts next to it beginning to have their weight supported by the vapour, which will insinuate itself under them, will be raised in their turn, till it either finds some vent, or is again condensed by the cold into water, and by that means prevented from proceeding any farther.

59. If a large quantity of vapour should continue to be generated for some time, several waves might be produced by it; and this would be, in some measure, the case, if the quantity at first generated was exceedingly great, though the whole of it was generated in less time, than whilst the motion was propagated through the distance between two waves.

60. These waves must rise the higher, the nearer they are to the place from whence they have their source; but, at great distances from thence, they may rise so little, and so slowly, as not to be perceived, but by the motions of waters, hanging branches in churches, &c.

61. The vibratory motion occasioned by the first impulse of the vapour, will be propagated through the solid parts of the earth, and therefore, it will much sooner become too weak to be perceived, than the wave-like motion; for this latter, being occasioned by the vapour insinuating itself between the strata,

strata, may be propagated to very great distances; and even after it has ceased to be perceived by the the senses, it may still discover itself by the appearances before-mentioned.

S E C T. IV.

62. All earthquakes derived from the same subterraneous fire, must come to the same place in the same direction; and those only which are derived from different fires, will come from different points of the compass; but as, in all probability, it seldom happens that earthquakes, caused by different fires, affect the same place, we therefore find in general, that they come from the same quarter: it is not, however, to be supposed, that this should always be the case, for it will, probably, sometimes happen to be otherwise: and this is to be expected in such places as are situated in the neighbourhood of several subterraneous fires; or where, being subject to the shocks of some local earthquake of small extent, they now and then are affected by an earthquake, produced by some more distant, but much more considerable cause. Of this last case, we seem to have had some instances in the earthquake of the 1st of November 1755, and those local ones, before-mentioned, which succeeded it.

63. As we may reasonably infer from many earthquakes coming to the same place, from the same point of the compass, that they are all derived from the same cause, and that a permanent one; so we may reasonably infer the same thing also, from their being propagated with the same velocity; but this argument will still come with the greater force, if it be considered,

considered, that the velocity of any vapour, which insinuates itself between the strata of the earth, depends upon the depth of it below the surface; for the deeper it lies, the greater will be its * velocity. We may therefore conclude, from the sameness of the velocity of the earthquakes of the same place, that the cause of them lies at the same depth; and from the inequality of the velocity of the earthquakes of different places, that their causes lie at different depths. Both these are perfectly consistent with the supposition, that earthquakes owe their origin to subterraneous fires, since the strata in which these subsist, may be easily conceived to lie at different depths in different parts of the world.

S E C T. V.

64. From the same cause, we may easily account for those local earthquakes, which succeed the greater and more extensive ones. If there are many subterraneous fires subsisting in different parts of the world, the vapour coming from one fire may very well be supposed, as it passes, to disturb the roof over some other fire, and, by that means, occasion earthquakes by the falling in of some part of it: and this may be the case, in some measure, even where the vapour passes at some small distance over the fire; but it will be most likely to take place, where the vapour either

* The velocity of such a vapour, depending intirely upon the elasticity of the earth which is over it, will be, *cæteris paribus*, (if I am not mistaken) in the ratio of the depth below the surface. This seems to follow from a known law of all elastic bodies, according to which they tend to return to their state of rest, when either dilated or compressed, with forces proportionable to the quantity by which they differ from their natural bounds.

passes at some distance under it, or between the stratum, in which the fire lies, and that next above or below it.

P A R T III.

S E C T. I.

65. **I**N the former part of this tract, I supposed a part of the roof over some subterraneous fire to fall in: this is an event that cannot happen merely accidentally; for so long as the roof rests on the matter on fire, no part of it can fall in, unless the matter below could rise and take its place: now, it is very difficult to conceive how this should happen, unless it was to rise by some larger passages than the ordinary fissures of the earth, which seem much too narrow for that purpose; for, besides that the melted matter cannot be supposed to have any very great degree of fluidity, it must necessarily have a hard crust formed upon it, at all the fissures, by the long continued contact of the water contained in them: these impediments seem too great to be overcome by the difference of the specific gravities of the part that is to fall in, and the melted matter, which is the only cause that can tend to make it descend; the manner therefore, in which, I suppose, this event may be brought about, is as follows:

66. The matter of which any subterraneous fire is composed, must be greatly * extended beyond its original

* As all bodies we are acquainted with are liable to be extended by heat, there can be no doubt of its being so in this case likewise

ginal dimensions by the heat. As this will be brought about gradually, whilst the matter spreads itself, or grows hotter, the parts over the fire will be gradually raised and bent; and this bending will, for some time, go on without any other consequence; but, as the fire continues to increase, the earth will at last begin to be raised somewhat beyond the limits of it. By this means, an annular space will be formed at the edges next to the fire, and surrounding it, a vertical section of which space, through a diameter of the fire, will be two long triangles, the shortest side or base of each lying next the fire, and the two longer sides being formed by the upper and lower strata, which will be separated for a considerable extent, proportionably to the distance through which they are raised from each other *. This space will be gradually

likewise; but the matter of subterraneous fires is yet much more extended, than those bodies which are only capable of being melted into a solid glass, if we may judge of it from what we see of volcanos; for the lavas, scori, and pumice stones, thrown out from thence, even after they are cold, are commonly of much less specific gravity, on account of their porous spongy texture, than the generality of earth, stones, &c. and they frequently are even lighter than water, which is itself lighter than any known fossil bodies, that compose strata in their natural state.

* In Fig. 4. A is supposed to represent a vertical section of the matter on fire; BB, parts of the same stratum yet unkindled; CC, the two sections of the annular space, (surrounding the fire) which is supposed to be filled with water, as far as the strata are separated; D, the several sets of earth, stones, &c. lying over the fire, which are raised a little, and bent, by the expansion of the matter at A. As it is not easy to represent the things above described in their due proportions, it may not be amiss, in order to prevent the figure here given from misleading the reader, to give some random measures of the several parts, such as may probably

gradually filled with water, as it is formed, the melted matter being prevented from filling it, by its want of fluidity, as well as on account of the other circumstances, under which it is to spread itself; for the lentor and sluggishness of this kind of matter is such, that, when somewhat cooled on the surface by the contact of the air only, it will not flow, perhaps, ten feet in a month, though in a very large body; instances of which we have in the lavas of *Héna*, *Vesuvius*, &c. It is not to be expected then, that it should spread far, when it comes in contact with water at its edges, as soon as it is formed, and when it is, perhaps, several months in acquiring a thickness of a few inches; but it must, by degrees, form a kind of wall between the fire and the opening into the annular space before described. This wall will gradually increase in height, till it becomes too tall in proportion to its thickness, to bear any longer the pressure of the melted matter; which

approach towards those which are sometimes found in nature: we may suppose then the stratum B to be, perhaps, from ten or twenty to a hundred yards in thickness; the greatest height of the annular space C, next the fire, to be from four or five to ten or fifteen feet, and its greatest extent, horizontally, from ten or twenty to fifty or sixty feet; the horizontal extent of the fire at A, may be from half a mile to ten or twenty miles; [See art. 29. and the note to art. 53.] and the thickness of the superincumbent matter at D, may be from a quarter or half a mile to two or three miles; the number of the laminae also, into which it is divided, may be many times more than those in the figure. As to the perpendicular fissures, they must be so numerous, and so small, in proportion to the other parts, that I chose rather to leave them, to be supplied by the imagination of the reader, than attempt to express them in a manner, that could give no adequate idea of them at all.

must

must necessarily happen at last, because the thickness of it will not exceed a certain * limit.

67. Besides the giving way of this wall, the fire may undermine the space containing the water, and, by that means, open a communication between them. Let us suppose one of these come to pass, and the time arrived when the partition begins to yield. If then the water had any way to escape readily, the breach would be made, and the melted matter would burst forth immediately, and flow out in large quantities at once amongst it; but as this is not the case, and it can only escape by oozing slowly between the strata, and through the fissures, the way that it came, the breach will be made gradually, from whence we may account for some appearances that have preceded great earthquakes.

68. We are told, that two or three days before an † earthquake in New England, the waters of some wells were rendered muddy, and stank intolerably :

* This limit will depend upon the thickness of the matter necessary to prevent so quick a communication of the heat or cold through it, as that the water should be able to diminish the heat of the fire considerably. The thickness requisite to do this, is very different in different kinds of bodies. Metals of all kinds transmit heat and cold extremely readily; but bricks and vitrified substances (with which last we may class the matter under our present consideration) transmit them very slowly: the walls of the hottest of our furnaces, when built of bricks, and eighteen inches thick, will not transmit more heat than a living animal can bear without injury, though the fires are continued in them for ever so long a time; probably, therefore, if we allow two feet for the thickness of the matter, cooled and rendered hard by the contact of the water, we shall not underdo it.

† See Philos. Trans. N^o 437. or Martyn's Abridgm. vol. viii. p. 689.

why might not this be occasioned by the waters contained in the spaces before described, which, being impregnated with sulphureous steams, were driven up, and mixed with the waters of the springs? At least, there can be no doubt, by whatsoever means it was brought about, that this phænomenon was owing to the same cause, already beginning to exert itself, which afterwards gave rise to the succeeding earthquake.

69. Something like this happened before the great Lisbon * earthquake of 1755. We are told, that at Colares, about twenty miles from thence, “ in the afternoon preceding the 1st of November, the water of a fountain was greatly decreased: on the morning of the 1st of November, it ran very muddy, and after the earthquake, it returned to its usual state, both in quantity and clearness.” The same author says, a little lower, “ in the afternoon of the 24th, I was much apprehensive, that the following days we should have another great earthquake; for I observed the same prognostics as in the afternoon of the 31st October; that is,” &c. “ And I farther observed, that the water of a fountain began to be disturbed to such a degree, that in the night it ran of a yellow clay colour; and from midnight to the morning of the 25th, I felt five shocks, one of which seemed to me as violent as that of the 11th of December.”

70. But the most extraordinary appearance of any that preceded this earthquake, was that of the agita-

* See *Philos. Trans.* vol. xlix, p. 416 and 417.—or *Hist. and Philos. of Earthq.* p. 313.

tion of the waters of * Lochness, and some others of the lochs in Scotland, about half an hour before any motion was felt at Lisbon, notwithstanding the cause of all these great effects could not lie far from thence, and, I think, certainly lay to the south of Oporto. Nor is it probable, that there should be any mistake in the time, not only because the difference is too great, as well as the concurrent testimonies too many, to admit of such a solution; but because they mention another greater agitation, that happened about an hour and half after the former; which latter agrees with the times, when the agitations of the waters were observed in England, if we allow only a proper interval for the motion to be propagated so far northward, proportionably to the time it took up in travelling from its original source near Lisbon.

71. These appearances seem to be connected with that mentioned in the preceding article, and they may both, I think, be accounted for, by supposing a considerable quantity of vapour to be raised, whilst the partition before-mentioned was beginning to give way; during which time, a partial

* See Philos. Transf. vol. xlix.—or Hist. and Philos. of Earthq: art. Lochness, Lochlommond, &c. The same thing also seems to have taken place in Switzerland; for Mons. Bertrand says, that all the agitations of the waters in the lakes there, which were observed on the 1st November 1755, happened between nine and ten in the morning; and particularly at lake Lemane, he says, the agitation happened just before ten; which, allowing for the difference of longitude, must have been just before nine at Lisbon; and, consequently, if there is no mistake in the times, all these agitations preceded the earthquake, at this last place, by near three quarters of an hour. [See *Memoires sur les tremblemens de Terre*, p. 107. et 105.]

communication between the water and fire would be brought on, and that by degrees only. Hence the vapour, not being produced at once but gradually, might creep * silently between the strata, towards that quarter where the superincumbent mass of earth was lightest; and, by this means, some places very near the source of the vapour might be little, or not at all, affected by it, whilst others might be greatly affected, though they lay at a great distance; and even those places, which lay immediately over the part where the vapour was passing, might not perceive any effect, on account of the gentleness of the motion, occasioned by the small quantity of it. This might continue to be the case, till it came to some country where, the set of strata above being much thinner, the vapour would not only be hurried forward, but collected also into a much narrower compass; and therefore, raising the earth more, would produce more sensible effects; and this we ought

* Some appearances that have been observed in New England seem to confirm this, and make it probable, that a small quantity of vapour is often found to creep silently between the strata, before a general communication between the water and the fire gives rise to the greater and more sensible effects of earthquakes. See *Philos. Transf.* N^o 462. or *Martyn's Abr.* vol. viii. p. 693. where we are told, that, at Newbury, a little before any noise or shock was perceived, the bricks of an hearth were observed to rise, and, falling down again, to lean another way. In the same account, it is also said, that "a few minutes before any shock came, many people could foretell it by an alteration in their stomachs:" an effect, which seems to be of the same kind with sea-sickness, and which always accompanies the wave-like motion of earthquakes, when it is so weak, as to be uncertainly distinguishable.

chiefly

chiefly to expect in the most mountainous countries, according to the idea before given of them *.

72. To make this something clearer, let us suppose, in Fig. 1. the vapour to be passing between the strata in the dotted line C, and to go forwards, till it arrives at A: whilst, then, it passes under the deeper parts at E, it will raise the earth over it but little, as well because it will be spread broader and thinner, as because it will be more compressed by the weight of the superincumbent matter; but as it arrives towards A, not only the latter part will be driven forwards with greater velocity, but the foremost will travel slower, on account of its travelling under a † thinner set of strata; and, besides this, the load being much less, it will greatly expand itself. From all these causes taken together, the wave at the surface of the earth, occasioned by the passing of the vapour under it, will not only be much higher, but also much shorter, and, consequently, the sides of it, on both these accounts, will be much more inclined to the horizon: and, moreover, because the progress of the wave will be slower, it will give more time to any waters situated on one side of it, to flow one way; and on this account also, the apparent agitation of them will be increased.

SECT. II.

73. We are told, that, in the Lisbon earthquake of 1755, “the bar [at the mouth of the Tagus] was “seen dry from shore to shore; then suddenly the sea,

* See art. 43.

† See art. 63. the note.

“ like a mountain, came rolling in ; and about Bel-
 “ lem castle, the water rose fifty feet almost in an
 “ instant ; and, had it not been for the great bay
 “ opposite to the city, which received and spread
 “ the great flux, the low part of it must have been
 “ under water *.” The same phenomena were ob-
 served to accompany the same earthquake at the island
 of Madeira ; where we are told, that, at the city of
 Funchal, “ the sea, which was quite calm, was ob-
 “ served to retire suddenly some paces ; then rising
 “ with a great swell, without the least noise, and as
 “ suddenly advancing, it overflowed the shore, and
 “ entered the city. It rose full fifteen feet perpen-
 “ dicular above high-water mark, although the tide,
 “ which ebbs and flows there seven feet, was then
 “ at half ebb. In the northern part of the island,
 “ the inundation was more violent, the sea retiring
 “ there above one hundred paces at first, and sud-
 “ denly returning, overflowed the shore, forcing
 “ open doors, breaking down the walls of several
 “ magazines and storehouses, and carrying away, in
 “ its recess, a considerable quantity of grain, and
 “ some hundred pipes of wine †.”

74. Both these appearances (which have been
 observed to attend several other earthquakes, as
 well as this) seem to admit of an easy solution, sup-
 posing the cause of them to lie under the bed of the
 ocean ; for, in the farther progress of the communi-
 cation between the fire and water, the vapour, that is

* See Hist. and Philos. of Earthq. p. 316.

† See Philos. Transf. vol. xlix. p. 432, &c.—or Hist. and Philos.
of Earthq. p. 329.

gradually raised at first, will at last begin to raise the roof over the fire, which, being supported by so light a vapour, there will now be no want of fluidity in the matter it rests upon, and the difference of specific gravity between the two, instead of being small, will be very great: hence, if any part of the roof gives way, it must immediately fall in, the vapour readily rising, and taking its place; and a beginning being once made, a communication will be opened with numberless clefts and fissures, that must occasion the falling in of vast quantities of matter, which, as soon as the vapour can pass round them, will want their support; then will follow the great * effects already described.

75. Now, whilst the roof is raising, the waters of the ocean, lying over it, must retreat, and flow from thence every way; this, however, being brought about slowly, they will have time to retreat so gently, as to occasion no great disturbance: but as soon as some part of the roof falls in, the cold water contained in the fissures of it, mixing with the steam, will immediately produce a vacuum, in the same manner as the water injected into the cylinder of a steam engine, and the earth subsiding, and leaving a hollow place above, the waters will flow every way towards it, and cause a retreat of the sea on all the shores round about: then presently, the waters being again converted by the contact of the fire into vapour, together with all the additional quantity, which has now an open communication with it, the earth will be raised, and the waters over it will be made to flow every

* See art. 56 to 60 inclusive.

way, and produce a great wave immediately succeeding the previous retreat *.

S E C T. III.

76. That great quantity of water, which we have supposed to be let out upon subterraneous fires, and, by that means, to produce earthquakes, will supply us with a reason, why they observe a sort of periodical return. This water must extinguish a great portion of the burning matter, in consequence of which, it will be contracted within much narrower bounds; and though the effects before described could not take place at first, but by the great extension of the heated matter, yet, after they have once taken place,

* It may, perhaps, be objected, that these phænomena may easily be occasioned by a vapour generated under the dry land, which, by first raising the earth upon the sea-shore, would make the waters retreat; and that the return of them again, upon its subsiding into its place, might cause the subsequent wave. That this may be the case, in some instances, is not impossible, but, I believe, upon examining the particular circumstances, it will generally be found to be otherwise; and there cannot be any doubt about it, in the case of the Lisbon earthquake; for the retreat was observed to precede the wave, not only on the coast of Portugal, but also at the island of Madeira, and several other places: now, if the retreat had been caused by the raising of the earth on the coast of Portugal, the motion of the waters occasioned by this means, when propagated to Madeira, must have produced a wave there previous to the retreat, contrary to what happened; nor could the motion of the waters at Madeira be caused by the earthquake at that place, because it did not happen till above two hours after; whence it is manifest, that it must have been owing to the continuation of a motion propagated from the place, where the earthquake exerted its first efforts. And we may observe, in general, that this must always be the case, whenever the retreat does not happen till some considerable time after the earthquake.

they

they may well continue to do so for some time; for the great disturbance in the first instance, by the falling in of a great part of the roof, must render the frequent communication between the fire and water not only very easy, but almost unavoidable: and this will continue to be so, till the roof is well settled, and the surface of the melted matter sufficiently cooled, after which, it may require a long time for the fire to heat it again so much, as will be necessary to make it produce the former effects. Now, as the matter has been more or less cooled, or as the combustible materials are with more or less difficulty set on fire again, as well as on account of other circumstances, the returns of these effects will be later or earlier; but though they will not, for this reason, observe any exact period, yet they will generally fall within some sort of limits, till either the matter that occasions them is consumed, (which, probably, will seldom happen in less than many ages) or till the fires open themselves a passage, and become volcanos.

S E C T. IV.

77. I have already intimated, that the most extensive earthquakes frequently take their rise from the sea. According to the description of the * structure of the earth before given, any combustible stratum must lie at greater depths in places under the ocean, than elsewhere; hence far more extensive fires may subsist there, than where the quantity of matter over them is less; for any vapour raised from such fires,

* See art. 43.

having

having both a stronger roof over it, and being pressed by a greater weight, (beside the additional weight of the water) will not only be less at liberty to expand itself, and consequently of less bulk, but it will also be easily driven away towards the parts round about, where the superincumbent matter is less, and therefore lighter. On the other hand, any vapour raised from fires, where the superincumbent matter is lighter, finding a weaker roof over it, and being not so easily driven away under strata, that are thicker and heavier, will be very apt to break through, and open a mouth to a volcano; and it must necessarily do this long before the fires can have spread themselves sufficiently, to be near equal to those which may subsist in places that lie deeper. All this seems to be greatly confirmed by the situation of volcanos, which are almost always found on the * tops of mountains, and those often some of the highest in the world.

78. If, then, the largest fires are to be supposed to subsist under the ocean, it is no wonder that the

* Perhaps this may supply us with a hint (if the conjecture is not thought extravagant) concerning the manner in which these mountains have been raised, and why the strata lie generally more inclining from the mountainous countries, than those countries themselves; an appearance not easily to be accounted for, but upon the supposition, that the upper parts of the earth rest upon matter, in some degree, though not perfectly fluid, and that this matter is lighter than the earth that rests upon it. This conjecture, however, will probably be thought less strange, if it be considered, that the new islands, formed about Santerini and the Azores, have some of them been raised from 200 to 300 yards, and upwards; a height which might well enough intitle them to the denomination of mountains, if they had been raised from lands not lying under the ocean. [See Fig. 3.]

most extensive earthquakes should take their rise from thence: the great earthquake of Lisbon has been * shewn to have done so; and that the cause of it was also at a greater depth, than that of many others, appears from the greater † velocity with which it was propagated.

79. The great earthquake that destroyed Lima and Callao in 1746, seems also to have come from the sea; for several of the ports upon the coast were overwhelmed by a great wave, which did not arrive till four or five minutes after the earthquake began, and which was preceded by a ‡ retreat of the waters, as well as that at Lisbon. Against this, it may, perhaps, be alleged, that there were four || volcanos broke out suddenly, in the neighbouring mountains, when this earthquake happened, and that the fires of these might be the occasion of it. This however, I think, is not very probable; for, to omit the argument of the wave, and previous retreat of the waters, already mentioned, it is not very likely, that more than one fire was concerned: besides, the vapour, opening itself a passage at these places, could not well be supposed, if it took its rise from thence, to spread itself far; especially towards the sea, where it is manifest,

* See art. 54. See also art. 94 to 97 inclusive.

† See the note to art. 63.

‡ Both the wave and previous retreat have been observed in the other great earthquakes, which have happened at Lima, and in the neighbouring country. See d'Ulloa's Voyage to Peru, part ii. book i. chap. 7.

|| If these volcanos were not new ones, but only old ones which broke out afresh, [See the note to art. 34.] the argument will come with still greater force.

that the strata over it were of great thickness, as appears from the great velocity with which the earthquake was propagated there: the shocks also continued with equal, or nearly equal violence, for some months after the openings were made; whereas, if these fires had been the cause of them, they must immediately have ceased, upon the fires finding a vent, as it has happened in other * cases. It is therefore much more probable, that a very large quantity of vapour, taking its rise from some far more extensive fire under the sea, spread itself from thence; and as it passed in places, where the roof over it was naturally much thinner, as well as greatly weakened by the undermining of these fires, it opened itself a passage, and burst forth.

80. As the most extensive earthquakes generally proceed from the lowest countries, but especially from the sea, so those of a smaller extent are generally found amongst the mountains: hence it almost always happens, that earthquakes, which are felt near the sea, if at all violent, are felt also in the higher lands; whereas there are many amongst the hills, and those very violent ones, which never extend themselves to the lower countries. Thus we are told, that, at Jamaica, “ † shakes often happen in
“ the country, not felt at Port-Royal; and some-
“ times are felt by those that live in and at the foot

* See art. 28.

† This is taken from an account of the earthquake that happened at Jamaica in the year 1692, which, as well as some others before-mentioned, was attended with the wave and previous retreat. See *Philos. Transf.* N^o 209. or *Lowthorp's Abr.* vol. ii. p. 417 and 418.

“ of the mountains, and by no body else.” On the other hand, the earthquake that destroyed Port-Royal extended itself all over the island: and the same was observed of a smaller earthquake, that happened there in 1687-8; which latter undoubtedly came from the sea, as appears by Sir * Hans Sloane’s account of it.

81. Earthquakes of small extent are also very common amongst the mountains of Peru and Chili. Antonio d’Ulloa says, “ Whilst we were preparing for
“ our departure from the mountain Chichi-Choco,
“ there was an earthquake which was felt four
“ leagues round about: our field tent was tossed to
“ and fro by it, and the earth had a motion like
“ that of waves; this earthquake, however, was
“ one of the smallest, that commonly happen in that
“ country.” The same author tells us, in another place, that, “ during his stay at the city of Quito,
“ or in the neighbourhood of it, there were two
“ earthquakes, violent enough to overturn some
“ houses in the country, which buried several persons
“ under their ruins.”

S E C T. V.

82. It is generally found, that earthquakes in hilly countries, are much more violent than those, which happen elsewhere; and this is observed to be the case, as well when they take their rise from the lower countries, as amongst the hills themselves. This appearance being so easily to be accounted for, from the structure of the earth already described, I

* See Phil. Transf. N^o 209, or Lowthorp’s Abr. vol. ii. p. 410.

shall content myself with establishing the certainty of a fact, which tends so greatly to confirm it.

83. The earthquakes that have infested some of the towns in the neighbourhood of Quito, have not only been incomparably more violent than that which destroyed Lisbon, but they seem to have exceeded that also which destroyed Lima and Callao. In * Lisbon, many of the houses were left standing, although few of them were less than four or five stories high. At Lima also, it is only said, that "all the buildings, great and small, or at least the greatest part of them, were destroyed." Callao likewise, as it appears from the accounts we have of it, had many houses left unhurt by the earthquake, till the wave came, which overwhelmed the whole town, and threw down every thing that lay in its way. All these effects seem to be greatly short of those produced by an earthquake that happened at Latacunga, in the year 1698, when the whole town, consisting of more than six hundred houses, was entirely destroyed in less than three minutes time, a part of one only escaping; notwithstanding that the houses there are never built more than one story high, in order, if possible, to avoid these dangers. Ambato, a village about the same size as Latacunga, together with a great part of Riobamba, another town in the same neighbourhood, were also entirely destroyed by the same earthquake, and some others were either destroyed, or received considerable damage

* See Philos. Transf. vol. xlix. p. 403. where it is said, "of the dwelling-houses, there might be about one fourth of them that tumbled."

from

from it. At the same time, a volcano burst out suddenly in the neighbouring mountain of Carguayrafo, as before-mentioned; and, "near Ambato, the earth
 " opened itself in several places, and there yet re-
 " mains, to the south of that town, a cleft of four
 " or five feet broad, and about a league in length,
 " lying north and south; there are also several other
 " like clefts on the other side of the river." The city of * Quito was affected at the same time, but received no damage, though it is no more than forty-two geographical miles from Latacunga, not far from whence the greatest violence of the shock seems to have exerted itself. These towns are supposed to stand by far the highest of any in the world, being as high above the level of the sea, as the tops of some of the highest mountains in Europe; and the ground upon which Riobamba stands, wants but † ninety yards of being three times as high as Snowdon, the highest mountain in Wales.

84. The country upon which these towns stand, serves as a base, from whence arise another set of high lands and mountains, which are much the highest in the known world. Amongst these mountains there are no less than six volcanos, if not more,

* The city of Quito stands lower than the level of Riobamba, by about 500 yards perpendicular. Though it escaped this, it has lately, however, been destroyed by another violent earthquake, that happened on the 28th April 1756, of which I have not yet seen any other particulars worth notice.

† This is according to Antonio d'Ulloa's account; but Mons. Condamine makes it exactly three times the height of Snowdon, computing it at 1770 toises. [See his measure of a degree of the meridian.]

within an extent of 120 miles long, and less than thirty broad, the lowest of which exceeds the height of Riobamba by above two thirds of a mile, and the highest by more than twice that quantity. Now, as the earthquakes have been more violent at the foot of these mountains, than in the lower lands, so they have been still more violent towards the tops of them: this is sufficiently manifest, from the many * rents made in them, and the rocks that have been broken off from them, upon such occasions: but it appears still more manifestly, and beyond all dispute, in the bursting forth of volcanos, which are almost always at the very † summit of the mountains, where they are found. In these instances, the earth, stones, &c. which lay over the fire, are generally scattered by the violence of the vapour, that breaks its way through, to the distance of some miles round about.

85. The great earthquake of the 1st November 1755, was also more violent amongst the mountains, than at the city of Lisbon. We are told, that “ the
“ mountains of Arrabida, Estrella, Julio, Marvan,
“ and Cintra, being some of the largest in Portugal,
“ were impetuously shaken, as it were, from their
“ very foundations; and most of them opened at
“ their summits, split and rent in a wonderful man-
“ ner, and huge masses of them were thrown down
“ into the subjacent vallies ‡.”

* See d'Ulloa's Voyage to Peru, part i. book vi. chap. 2.

† The only exceptions that I know of to this rule, are in those cases, where the highest part having an opening already, some fresh mouth opens itself in the side of the mountain.

‡ See Hist. and Philos. of Earthq. p. 317.

86. The same was observed at Jamaica likewise. In the earthquake that destroyed Port-Royal in 1692, we are told, that “ more houses were left standing
 “ at that town, than in all the island besides. It was
 “ so violent in other places, that people were violently
 “ thrown down on the ground, where they lay with
 “ their legs and arms spread out, to prevent being
 “ tumbled about by the incredible motion of the
 “ earth. It scarce left a planter’s house or sugar-
 “ work standing all over the island: I think it left
 “ not a house standing at Passage fort, and but one
 “ in all Liganee, and none in St. Iago, except a few
 “ low houses, built by the wary Spaniards. In Cla-
 “ rendon precinct, the earth gaped, and spouted up,
 “ with a prodigious force, great quantities of water
 “ into the air, twelve miles from the sea; and all
 “ over the island, there were abundance of openings
 “ of the earth, many thousands. But in the moun-
 “ tains, are said to be the most violent shakes of all;
 “ and it is a generally received opinion, that the
 “ nearer to the mountains, the greater the shake;
 “ and that the cause thereof, whatever it is, lies
 “ there. Indeed they are strangely torn and rent,
 “ especially the blue, and other highest mountains,
 “ which seem to be the greatest sufferers, and which,
 “ during the time that the great shakes continued,
 “ bellowed out prodigious loud noises and echo-
 “ ings.

87. “ Not far from Yallowes, a mountain, after
 “ having made several moves, overwhelmed a whole
 “ family, and a great part of a plantation, lying a
 “ mile off; and a large high mountain near Port-
 “ morant,

“ morant, near a day’s journey over, is said to be
 “ quite swallowed up.

88. “ In the blue mountains, from whence came
 “ those dreadful roarings, may reasonably be sup-
 “ posed to be many strange alterations of the like
 “ nature; but those wild desert places being very
 “ rarely, or never visited by any body, we are yet
 “ ignorant of what happened there; but whereas
 “ they used to afford a fine green prospect, now one
 “ half part of them, at least, seem to be wholly de-
 “ prived of their natural verdure *.”

S E C T. VI.

89. I have supposed, that fires lying at the greatest depths generally produce the most extensive earthquakes, we must, however, except from this rule those cases where the depths are very great: for, as the weight of three miles perpendicular of common earth is capable of absolutely repressing the vapour of inflamed gunpowder, so we may well suppose, that

* See Philos. Transf. N° 209. or Lowthorp’s Abridg. vol. ii. p. 416, &c. where there is a great deal more to the same purpose. See also Hist. and Philos. of Earthq. p. 286 and 287.

From the authorities quoted in this section, it appears, how little reason there is for the notion, that either large cities, or towns situated near the sea-coast, are more subject to violent earthquakes than others: it is not, however, much to be wondered at, that such a notion should have prevailed, after the great destruction that happened in so large and populous a city as Lisbon; since the demolition of a few ruinous houses only, in such a place, would have affected the imaginations of men more, and would have been more talked of, than the subversion of whole mountains in some wild and desert country, where at most half a dozen unknown shepherds might feel the effects of it, or perhaps only see it at a distance.

there

there may be a quantity of earth sufficient to repress the vapour of water, and keep it within its original limits, though ever so much heated. Now, whenever this is the case, it is manifest, that it can produce no effect: or, it may happen, that though the quantity of earth may not be sufficient absolutely to repress the vapour, yet it may be so great, as to suffer it to expand but very little: in this case, an earthquake arising from it would be but of small extent; the wave-like motion would be little or none; the vibratory motion would be felt every-where; and the propagation of the motion would be very quick. This last circumstance being almost the only one, by which these earthquakes can be known from those which owe their origin to shallower fires, it must be very difficult to distinguish them with certainty, as it is almost impossible to distinguish the difference of the time of their happening in different places, when the whole, perhaps, is comprehended within the space of two or three minutes; possibly, however, some of the earthquakes, which we have had in England, may have been of this class.

S E C T. VII.

90. If we would inquire into the place of the origin of any particular earthquake, we have the following grounds to go upon.

91. *First*, The different directions, in which it arrives at several distant places: if lines be drawn in these directions, the place of their common intersection must be nearly the place sought: but this is liable to great difficulties; for there must necessarily be great uncertainty in observations, which cannot, at
best,

best, be made with any great precision, and which are generally made by minds too little at ease to be nice observers of what passes; moreover, the directions themselves may be somewhat varied, by the inequalities in the weight of the superincumbent matter, under which the vapour passes, as well as by other causes.

92. *Secondly*, We may form some judgment concerning the place of the origin of a particular earthquake, from the time of its arrival at different places; but this also is liable to great difficulties. In both these methods, however, we may come to a much greater degree of exactness, by taking a medium amongst a variety of accounts, as they are related by different observers. But,

93. *Thirdly*, We may come to the greatest degree of exactness in those cases, where earthquakes have their source from under the ocean; for, in these instances, the proportional distance of different places from that source may be very nearly ascertained, by the interval between the earthquake and the succeeding wave: and this is the more to be depended on, as people are much less likely to be mistaken in determining the time between two events, which follow one another at a small interval, than in observing the precise time of the happening of some single event.

94. Let us now, by way of example, endeavour to inquire into the situation of the cause, that gave rise to the earthquake of the 1st of November 1755, the place of which seems to have been under the ocean, somewhere between the latitudes of Lisbon and Oporto, (though probably somewhat nearer to
the

the former) and at the distance, perhaps, of ten or fifteen leagues from the coast. For,

95. *First*, The direction, in which the earthquake arrived at Lisbon, was from the north-west; at Madeira it came from the north-east; and in England it came from the south-west; all of which perfectly agree with the place assumed*.

96. *Secondly*, The times in which the earthquake arrived at different places, agree perfectly well also with the same point. And,

97. *Thirdly*, The interval between these, and the time of the arrival of the subsequent wave, concur in confirming it. That all this might appear the better, I have subjoined the following table, assuming the point, from whence I compute, at the distance of about a degree of a great circle from Lisbon, and a degree and half from Oporto. In consequence of this supposition, I have added three minutes to the interval between the time when the shock was felt at Lisbon, and at the several other places. The first column in the table contains the names of places; the second, the distances from the assumed point, reckoned in half degrees; the third, the time that the earthquake took up in travelling to each, expressed in minutes; and the fourth contains the time in which the wave was propagated, from its source to the respective places, expressed in minutes likewise.

* All these directions, together with the times when the earthquake, as well as the succeeding wave, arrived at different places, (two or three only excepted) are taken from the 49th volume of the *Philos. Transf.* and the *Hist. and Philos. of Earthq.* To these, I must refer the reader for the particular authorities, which, as they are very numerous, I was not willing to quote at length.

	Half deg.	Min.	Min.
Lisbon * - - - - -	2	3	12
Oporto * - - - - -	3	5	
Ayamonte - - - - -	6		53
Cadiz - - - - -	9	12	82
Madrid - - - - -	9	11	
Gibraltar - - - - -	11	18	
Madeira - - - - -	19	25	152
Mountsbay - - - - -	20		267
Plymouth - - - - -	21		360
Portsmouth - - - - -	23	29	
Kingfale - - - - -	23		290
Swansea - - - - -	24		530
The Hague - - - - -	30	32	
Lochness - - - - -	33	66	
Antigua - - - - -	98		565
Barbadoes - - - - -	101		485

98. In computing the times in the preceding table, allowance was made for the difference of longitude, as it is laid down in the common maps, which are

* It appears, by all the accounts, that the interval between the earthquake and wave, either at Oporto or Lisbon, was not long: I have met with no account yet, however, which tells us how long it was at the former, and only one which mentions it at the latter, where it is said to have been nine minutes. [See *Memoires sur les tremblemens de Terre*, p. 245. compared with *Hist. of Earthquakes*, p. 315.] These intervals, if we knew them exactly, might have served, perhaps, to ascertain the distance of those two places from the original source a little more accurately; but, as the distance of neither from thence could be very great, a small difference in them would hardly sensibly affect any of the others; from which, therefore, we may draw the same general conclusions, as if they were exact.

not

not always greatly to be depended on. The times themselves also are often so carelessly observed, as well as vaguely related, that they are many of them subject to considerable errors; the concurrent testimonies, however, are so many, that there can be no doubt about the main point; and, that the errors might be as small as possible, I have not only endeavoured to select those accounts that had the greatest appearance of accuracy, but, in all cases where it was to be had, I have always taken a mean amongst them. In many of the accounts, the relaters say only between such hours, or about such an hour: of this kind were the accounts of the times of the agitation of the waters at The Hague and Lochness, which vary the most from a medium of the rest, the former erring about seven minutes in defect, and the latter about twenty minutes in excess; with regard to the latter, however, I must observe, that, from the account itself, it is probable the agitation happened sooner than eleven o'clock, which is the time mentioned. The accounts also of the time of the agitation of the waters in the northern parts of England, seem to confirm the same thing*.

99. It is observable, in the preceding table, that the times, which the wave took up in travelling, are

* As the shortest way that the vapour could pass from near Lisbon to Lochness was under the ocean, possibly it might, on that account, be somewhat retarded; for the water adding to the weight of the superincumbent mass, and not to its elasticity, must produce this effect in some degree; it is probable, however, that this could make no great difference, as the motion seems to have been very little retarded in its passage from the original source to Madeira, to which place, I suppose, it must have passed under deeper seas than would be found in its road to Scotland.

not in the same proportion with the distances of the respective places from the supposed source of the motion; this, however, is no objection against the point assumed, since it is manifest, wherever it was, that it could not be far from Lisbon, as well because the wave arrived there so very soon after the earthquake, as because it was so great, rising, as we are told, at the distance of three miles from Lisbon, to the height of fifty or sixty feet. The true reason of this disproportion, seems to be the difference in the depth of the water; for, in every instance in the above table, the time will be found to be proportionably shorter or longer, as the water through which the wave passed was * deeper or shallower. Thus the motion of the wave to Kingfale or Mountsbay (through waters not deeper in general than 200 fathoms) was slower than that to Madeira, (where the waters are much deeper) in the proportion of about three to five; and it was slower than that to Barbadoes, (where its course lay through the deepest part of the Atlantic ocean) nearly in the proportion of one to three: so likewise the motion of it from the Scilly islands to Swansea in Wales (where the depth gradually diminishes from about sixty or seventy fathoms to a very small matter) was still slower than that to Kingfale, in the proportion of less than one to three: the same thing is observable with regard to

* We have an instance to this purpose in the tides, which, in deep waters, move with a velocity that would carry them round the whole earth in a single day; but as they get into shallower waters, they are greatly retarded: and we are told, that in the river of Amazons, the same tide is found running up to the tenth or twelfth day, before it is entirely spent. [See *Condamine's Voyage down the Maranon.*]

Plymouth also, where the wave arrived about ninety minutes later than at Mountsbay, though the difference of their distance from the first source could not, upon any supposition, be more than forty or fifty miles.

S E C T. VIII.

100. If we would inquire into the depth, at which the cause lies, that occasions any particular earthquake, I know of no method of determining it, which does not require observations not yet to be had; but if such could be procured, and they were made with sufficient accuracy, I think some kind of guess might be formed concerning it: for,

101. *First*, In those instances, where the vapour discharges itself at the mouths of volcanos, (as in the case of the earthquake at Lima) it might, perhaps, be possible for a careful observer to trace the * thickness of the several strata from thence to the place where the earthquake took its rise, or at least as far as the shore, if it took its rise from under the sea. If this could be once done in any one instance, and the velocity of such an earthquake nicely determined, we might then guess at the depth of the cause in other earthquakes, where we knew their velocity, by taking the † depths proportional to those velocities, which probably would answer very nearly.

102. *Secondly*, If, in any instance, it should be possible to know how much the motion of any earthquake was retarded by passing under the ocean, the

* This is upon the supposition, that the under strata, in ascending up the hills, come to the day in the manner before described. See art. 43. and Fig. 3.

† See the note to art. 63.

depth of the ocean being known, the depth at which the vapour passed would be known also; for the velocity under the water would be to the velocity, if there had been no water, in the subduplicate ratio of the weight in the latter case to the weight in the former; hence allowing earth to be about two and half times the weight of water, the depth will be readily found.

103. *Thirdly*, Let us conceive the earth to be formed according to the idea before given of it, and that the same strata are at a medium of the same thickness for a very great extent, as well in those places, where several of the upper ones are wanting, as where they are not. Upon this supposition, we may discover the depth, at which the vapour passes, by comparing the several velocities of the same earthquake in places, where the * thicknesses of the superincumbent mass are different. It must be acknowledged, indeed, that such observations with regard to time, as would enable us to determine these velocities, are in general much too nice to be expected: the matter, however, is not altogether desperate, as we may collect them, in some measure perhaps, from other circumstances, such, for instance, as the degree of † agitation in different waters, the proportional ‡ suddenness, with which the earth is lifted in different places, &c.

104. As

* In order to know this difference, it will be necessary to trace the thickness of those strata, which are found in some of the places, but are wanting in others.

† See art. 71 and 72.

‡ This may be known from the distance to which the mercury subsides in the barometer, upon the first raising of the earth by the vapour.

104. As the observations relating to the earthquake of the 1st of November 1755 are too gross, it would be in vain to attempt, by any of the foregoing methods, to determine with any certainty the depth at which the cause of it lay; but, if I might be allowed to form a random guess about it, I should suppose, (upon a comparison of all circumstances) that it could not be much less than a mile, or a mile and half, and I think it is probable, it did not exceed three miles.

C O N C L U S I O N.

105. Thus have I endeavoured to shew how the principal phænomena of earthquakes may be produced, by a cause with which none, that I have seen, appear to me to be incompatible. As I have not knowingly misrepresented any fact, so neither have I designedly omitted any that appeared to affect the main question; but, that I might not unnecessarily swell what had already much exceeded the limits at first intended for it, I have omitted,

106. *First*, Those minuter appearances, which almost every reader would easily account for, from what has been said already, and which did not seem to lead to any thing farther: such, for instance, are the sudden stopping and gushing out of fountains, occasioned by the opening or contracting of fissures; the dizziness and sickness people feel, from the almost imperceptible wave-like motion, &c.

vapour. I don't find, that this phænomenon, which is a common attendant on earthquakes, was observed any-where, at the time of the earthquake of the 1st of November 1755, except at Amsterdam, where the mercury subsided more than an inch. See Hist. and Philos. of Earthq. p. 309.

107. *Sec-*

107. *Secondly*, Those appearances which seemed to depend upon particular circumstances, and of which, therefore, unless we had a more exact knowledge of the countries where they happened, it would have been impossible to give any account, without having recourse to uncertain conjectures; of this kind, was the greater agitation of the waters in the lakes of Switzerland, at the time of the earthquake of the 1st of November 1755, than during the * earthquake of the 9th of December following, though the houses upon the borders of them were more violently shaken by the latter. And,

108. *Lastly*, Those appearances, which only seem to have an accidental connection with earthquakes, or the causes of them; of this kind, are the effects which, in some instances perhaps, they produce on the weather; the distempers which are sometimes said to succeed them; the disturbance which, we are told, they have sometimes occasioned, during the shocks, in the direction of the magnetic needle, &c. none of which are observed to be constant attendants on earthquakes, nor do they seem materially to affect the solution given either one way or other.

* See Monsieur Bertrand's *Memoires sur les tremblemens de Terre*.

LVI. *An Extract of a Letter from Abbé De la Caille, F. R. S. and Member of the Royal Academy of Sciences at Paris, to Matthew Maty, M. D. and F. R. S.*

Paris, Feb. 18, 1760.

Read March 6, 1760. **I**'LL venture to send you some of my observations on the present comet, because bad weather may have prevented it from being seen in England.

	Equal time.	Longitude.	North latitude.
	^h ['] ["]	[°] ['] ["]	[°] ['] ["]
Feb. 8.	9 29 28	Ω 20 17 22	3 26 42
9.	8 48 50	18 49 18	4 46 28
11.	7 22 35	16 5 3	7 14 50
13.	7 47 4	14 43 21	8 24 18
14.	6 41 0	12 20 18	10 30 9

These observations, together with another made at Marseilles, (on the first day) at 9^h 55' 38" equal time, when the longitude of the comet was found in Ω 23° 29' 46", and its north latitude 31' 20", have enabled me to compute the elements of its orbit. Its motion is direct. The ascending node is in Ω 19° 42' 0", and the place of the perihelion in ☿ 26° 41' 22". The inclination of the orbit is 80° 51' 30", and the distance of the perihelion $\frac{85248}{100000}$ of the radius of the orbit of the earth. The comet passed the perihelion Nov. 25, 1759, at 20^h 55', mean time, at Paris. These computations will be further improved by the observations I still hope to make; but they are sufficient to find the comet's place in the heavens.

LVII. *Extracts of some Letters from Signor
Abbate de Venuti, F. R. S. to J. Nixon,
A. M. and F. R. S. relating to several
Antiquities in Italy.*

Dear Sir,

Rome, Sept. 27, 1759.

Read March 20, 1760. — **S**OME days ago a fire accidentally broke out, where the Antonine column lay, near the Curia Innocenziana on Monte Citorio. It burnt the wooden props or scaffolding, which supported the column, and did such considerable damage to the column itself, by causing its surface to shell off, that it will be very difficult to set it up, if any attempt should be made for that purpose hereafter.

A certain Greek epigram, lately dug up at Naples, relating to a boy, has occasioned several grammatical altercations among the literati in that part of the world; and these again have produced some smart lampoons and satirical writings on each side of the question.

Many singular statues and relievos, lately discovered, have been set up in the gardens of Cardinal Albani, without the Porta Salaria. These, if you were among us, would afford a very agreeable entertainment to your curiosity.

LETTER

LETTER II.

Rome, Dec. 31, 1759.

—I have already met with some dissertations of Mr. Swinton, which shew his acute judgment, and excellent learning in the oriental languages; and therefore, I do not question his accuracy in treating of the epocha of the Sidonians. Your extract of his essay upon that subject has induced me to examine some coins stamp'd at Sidon, which I think it needless to describe, as the world is already in possession of their history. However, I have taken particular notice of one of that kind, of the smallest size, which, I think, has never been published. It exhibits the trunk of a palm-tree, or rather the *laseriptium* of Cyrene, with this legend, . On the reverse, there is a caldron, or vase, with these letters round it, . If Mr. Swinton thinks this coin will be of any service to him in his inquiry, I will send a more exact copy of it.

I come now (in compliance with your request) to give you some inscriptions lately dug up among the ruins of antient Rome.

The first was discovered without the Porta Latina, two miles from the city, and runs thus:

LIBERTI. LIBERTAE
PRISCI. AVG. L. GAMIANI
EX. TESTAMENTO. DESCRIPTV. ITA. VT
CAVTVM. ERAT. SVBSCIPSER
BOCVS. MONIMENTI. SIVE. AGER. EST. VIA. LATIN
AD. MILLIARIVM. V. SVpra. PONTEM. AD
MONIMENTVM. GAMI. CAESARIS. AGATHOCLIAN

DO. LEGO. LIBERTIS. MEIS. VTRIVSQVE. SEXSVS
 POSTERISQVE. EORVM. ET. IIS. QVOS. MANV
 MITTI. ROGAVI. IBIQVE. TABERNAM. FIERI.
 INQVE. EAM. REM. CONSVMI. HS. N̄.
 ARBITRATV. AGATHANGELI. COLLIB.

II.

The second is in my possession ; and is as follows :

D. M.
 SOTER. SERVOS. PVBLICVS.
 CASTELLAR. AQVAE. ANNIONIS
 VETERIS. FECIT. CONGVGI. BENE
 MERENTI. ET. L. CALPVARNIVS
 FLAVIANVS. MATRI. BENE
 MERENTI. SIBI. ET. SVIS
 POSTERISQVE. EORVM.

III.

On a Hermes, without a head.

D. M. V. F.
 SEX. SAMNIVS
 SEX. F. VVLTVR
 APER.
 DOMO. NEMAVSO
 IIIIVIR. IVREDICVND
 PONTIFEX. PVBLICORV
 SACRIFICIORVM
 PRAEFECTVS. VIGILVM
 ET. ARMORVM.

IV.

On the basis of a statue dressed in a toga, are the following letters :

EVENTIVS
 P. D. S. C.

I have

I have lately bought a relievo, which, in my opinion, is very singular. It is of marble, two palms wide, and one high, and represents, in a neat taste, a Faun, with a tail, and wings; which latter circumstance has never occurred to my observation before. He seems to be dancing, and his dog at his feet is in the same attitude. Near him is a tree, to which is tied a very elegant open chariot, (*thenfa*) and beneath it there appears a table, such as are used in entertainments, with a goblet upon it, charged with relievo in embossed work.

I have likewise, a few days ago, met with a cornelian, on which was engraved a man cloathed with a pallium, and sitting on a chair: before him there appeared a lighted furnace, and upon it a vessel of glass, or earthen ware. The artist himself held in his hand a pair of iron pincers, with handles, to take off the vessel from the fire, without burning himself. They demanded ten sequins for it, which I thought too great a price, and therefore did not purchase it.

Remarks on the preceding Extracts; in a Letter to Charles Morton, M. D. Secretary to the Royal Society: By John Nixon, A. M. F. R. S.

Dear Sir,

Read March 27, 1760. **I** Beg leave, in pursuance of my promise, to transmit to you some observations, which tend, in my opinion, to illustrate the preceding extracts of the Abbate de Venuti's letters to me.

The

The column, mentioned in Letter I. is of red granite, and was dug up (together with its pedestal) by Clement XI. out of the ruins on Monte Citorio, *A. D.* 1705. Innocent XII. intended to have set it up; but the great damage it had suffered in clearing the rubbish from it, hindered that design from being effected.

In 1745, Benedict XIV. removed it from the Piazza di Monte Citorio to its present situation on the same hill, and also caused the pedestal, that belonged to it, to be erected where it now stands, being the spot designed for it by his predecessors, mentioned above.

The relievi on the sides of the pedestal refer to the apotheosis of Antoninus Pius; as the inscription shews, that the column was erected to the memory of that emperor by M. Aurelius (stiled in this, as in some other antient monuments *, Antoninus) and L. Verus, his adopted sons, and associates in the government.

Letter II. inscript. 2. l. 2.—SERVOS. PVBLICVS.]
i. e. One of those officers, who were to attest the signing of all public instruments—*Censores ex templo jam atrium libertatis ascenderunt, et ibi signatis tabellis publicis, clausoque tabulario, et dimissis jervis publicis, negarunt se prius quidquam negotii publici gesturos, quam judicium populi de se factum esset.* Liv. Hist. l. xliii. c. 16. *A. U.* 584. Vid. Marcell. Donat. in loc.

* Vid. Bianchini apud Vasi delle Magnif. di Roma, lib. ii. p. 13, 14.

L. 3.—CASTELLAR.] For explaining this title, it is to be observed, that the reservoirs, which received the waters brought by aqueducts into the city of Rome, were called *castella*.

Agrippa in ædilitate suâ adjectâ virgine aquâ, cæteris corrivatis atque emendatis, lacus septingentos fecit: præterea salientes centum quinque; CASTELLA centum triginta, complura etiam cultu magnifica. Plin. Hist. Nat. l. xxxvi. c. 15.

There are still extant very considerable remains of some of these *castella*, viz. that of Aqua Claudia at Porta Maggiore; and another of Aqua Marcia, commonly called I Trofei di Mario, near St^o Eusebio in Rome.

From hence it is obvious to collect, that the office of the *castellarius* was to superintend the public reservoirs above-mentioned, to distribute the waters in due proportion into the several quarters of the city, &c. Fabretti has preserved an inscription, relating to one of these officers, viz. the *castellarius* of the Aqua Marcia. *Vid. Bianchini apud Vasi delle Magnif. di Roma, lib. i. p. 31.*

Ib. L. 3.—AQVAE. ANNIONIS. VETERIS.] This water was brought to Rome A. U. 481. It was taken up from the Anio, or Teverone, above Tibur, (Tivoli) about twenty miles from Rome, and entered the city by the same aqueduct, which conveyed the Aqua Appia, near the Porta Capena (St^o Sebastiano). From thence it passed along between M. Aventinus and Cælius, to the end of the Vicus Publicius, near the modern Scola Greca, where it had its reservoir contiguous to that of the Aqua Appia
above-

above-mentioned. *M. Angel. Rossi Roma Antica*, p. 229. 231.

The epithet *vetus* distinguishes this water from that of the *Anio novus*, which was brought to Rome from the distance of sixty-two miles, by the emperor Claudius, in the twelfth year of his reign, about *A. U.* 803, as appears from an inscription in Vasi, lib. i. p. 32. To which we may add the testimony of Suetonius—(*Claudius*) *Rivum Anienis novi lapideo opere in urbem perduxit, divisitque in plurimos et ornatissimos lacus.* Suet. Claud. c. xx.

Inscript. 3. l. 7, 8.—PONTIFEX. PVBLICO-RV. SACRIFICIORVM.] If this title implies the same as that of the *Sacerdotes Publici* in Livy, it comprehended the several orders of Pontifex Maximus, Pontifex, Augur, Decemvir Sacrorum, &c. Liv. Hist. lib. xxv. c. 2. xxvi. 23. xli. 21, &c. But to which of these classes SAMNIVS, mentioned in this inscription, belonged, we cannot determine.

Ib. l. 9.—PRAEFECTVS. VIGILVM.] Some writers refer the first institution of a night-watch, for the safeguard of the city of Rome from fires, to Augustus. *Augustus adversus incendia excubias nocturnas, vigilesque commentus est.* Suet. Aug. c. xxx. Ἐπειδὴ ἐν τῷ χρόνῳ τῷ τῷ, &c. *Cum eo tempore multa essent in urbe incendiis vastata, libertinos septem locis ad ea curanda instituit (Augustus).* Dion. Cass. l. lv. *A. U.* 759. But Livy mentions an office subsisting in Rome, in the time of the republic, as early as *A. U.* 449, viz. the *Triumviratus nocturnus*, (Liv. Hist. lib. ix. c. 46.) which must have been the same

same in effect with that described above: for it must have been undoubtedly the province of those *Triumviri* to prevent all manner of disorders and accidents in the night, especially that most calamitous one of fires. This conclusion, in general, results from the nature of things; as the particular circumstance relating to fires may be collected from Tacitus. — *Triumviri, quòd ad incendium in sacrâ viâ ortum tarde venerant, à tribuno plebis die dictâ damnati.* [Tac. apud Godeler. not. in Liv. supra.] Now, in order to reconcile these accounts, in some degree, we may say, that, as the inhabitants of Rome were become more numerous and wealthy, and (consequently) more licentious, in Augustus's time, that emperor found it requisite to new model the night-watch, by making several additional improvements in it, with regard, probably, to the number and condition of the persons, of whom it consisted; the quality of the officer, who presided over it; the appointment of the proper places of rendezvous in the most commodious parts of the city, &c. And, upon account of these regulations made by Augustus in the night-watch, some historians may have been induced to consider him as the founder of this police in the city of Rome.

I am, &c.

J. Nixon.

LVIII. *A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the worshipful Company of Apothecaries, for the Year 1759, pursuant to the Direction of Sir Hans Sloane, Baronet, Med. Reg. & Soc. Reg. nuper Præses, by John Wilmer, M. D. clariss. Societatis Pharmaceut. Lond. Socius, Hort. Chelsean. Præfectus & Prælector. Botanic.*

Read March 27, } 1851 **A**CER foliis trilobis integerri-
1760. } mis. Flor. Leyd. Prod. 459.

Acer trifolium. C. B. P. 431.

1852 *Alfine marina* foliis portulacæ. Fl. Pruff. P. 12.
Ic. 2.

Anthyllis maritima centifolia. C. B. P. 282.

Alfine centifolia, seu Alfine cruciata marina.
Ger. Emac. 622.

1853 *Aster* foliis lanceolatis acutis scabris, caule simplici, floribus umbellatis terminalibus. Dict. Hort.

Aster ramosus annuus Canad. Mor. Hist.

1854 *Azalea* ramis infra flores foliosis. Flor. Virg.

1855 *Brassica* foliis infimis ovato-lanceolatis, serrato-dentatis, summis linearibus integerrimis.

1856 *Bromos* panicula nutante, foliis utrinque subpilosis, glumis ciliatis. Lin. Sp. Pl. 76.

1857 *Calendula* foliis obverse ovatis denticulatis, caule fruticoso perenni. Flor. Leyd. Prod. 531.

1858 *Cerasus* foliis ovatis. Virg. Hort. Cliff. 43.

Cerasus

Cerasus Sylvestris amara Mahaleb putata. I. B.
I. 227.

1859 *Chelidonium maximum* Canadense acaulon
Cornuti.

Sanguinaria. Hort. Eltham.

1860 *Clematis compositis et decompositis foliolis*
ternatis ferratis. Gmel.

1861 *Clematis purpurea repens*, petalis florum co-
riaceis. Banister.

1862 *Cratægus*; folio subrotundo, ferrato, subtus in-
cano, Aria multis. Tourn. 633.

1863 *Cratægus*; *Cerasi* folio, floribus magnis. Am-
man. Ruth. 274.

1864 *Cratægus*; folio laciniato. Tourn. 633.
Sorbus torminalis. Dod. 803.

1865 *Dens canis* latiore; rotundioreque folio, flore
ex purpura rubente. C. B. 87.

1866 *Elymus spica erecta*, spicula involucro lon-
giore. Hort. Upsal. 22.

1867 *Erigeron foliis basi revolutis*. Lin. Sp. Pl. 863.
Senecio Bonariensis purpurascens, foliis imis
coronopi. Hort. Eltham. 344.

1868 *Eruca latifolia*, alba sativa, *Doscoridis*. C. B.
98. Officin. 185.

1869 *Euonymus floribus omnibus quinquefidis*. Lin.
Sp. Pl. 197.

Euonymus Virginianus pyracanthæ foliis, cap-
sula verrucarum instar exasperata. Pluk.
Almag. 139.

1870 *Gypsophylla foliis lanceolato-linearibus obso-*
lete triquetris lævibus obtusis. Lin. Gen.
nov. 1103.

- Saponaria petalis* ovatis, foliis glaucis pulposis.
Hall. Jen. 117.
- 1871 *Hypericum foetidum frutescens majus*. Tourn.
Hypericum frutescens Canariense multiflorum.
Hort. Amst.
- 1872 *Hypericum foetidum frutescens*. Tourn.
Tragium legitimum veterum. Ponæ Baldo 88.
- 1873 *Hypericum orientale foetido simile sed inodorum*. T. Cor.
- 1874 *Jasminum foliis lanceolatis oppositis integerri-
mis calycibus acutioribus*. Miller's Icons.
- 1875 *Ixia planifolia, caule multiflora, spatha brevif-
sima*. Miller.
- 1876 *Lunaria major; filiqua rotundiore*. I. B. 2.
881.
- 1877 *Lycopus foliis æqualiter ferratis*. Lin. Sp.
Plant. 21.
Lycopus foliis lanceolatis tenuissime ferratis.
Flor. Virg. 8.
- 1878 *Majorana rotundifolia scutellata exotica*. H. R.
Par.
- 1879 Ortega. Læfl. epist. viii. Lin. Sp. Pl. 560.
Juncaria Salmatica. Clus. Hist. 1. P. 174.
- 1880 *Osteospermum foliis oppositis palmatis*. Lin.
*Chrysanthemum perenne Virginianum majus
platani folio orientale*. Mor. Hist.
- 1881 *Oxalis scapo umbellifero, foliis ternatis biparti-
tis*. Lin. Sp. Pl. 434.
*Oxalis bulbosa pentaphylla et hexaphylla, flo-
ribus magnis luteis et copiosis*. Burman.
Plant. Afric. 80.
- 1882 *Peucedanum Germanicum*. C. B. P. 149.
Mor. Umb. 36.

Fœniculum

- Fœniculum porcinum.* Lobel. Advers. 350.
- 1883 *Potentilla foliis bipinnatis, segmentis integerrimis distantibus subtus tomentosis, caule decumbente.* Lin. Sp. Pl. 496.
Pentaphylloides supinum minus, folio glabro non ferrato. Amman. Ruth. 116.
- 1884 *Robinia pedunculis simplicissimis, foliis abrupte pinnatis.* Hort. Upsal. 212.
Caragana Siberica. Flor. Leyd. 537.
- 1885 *Salvia foliis oblongo-ovatis integerrimis calycibus patulis coloratis.* Fig. Tab. 225. F. 2.
- 1886 *Scabiosa Africana frutescens, foliis rugosis, et crenatis minor.* Par. Bat.
- 1887 *Seseli caule alto rigido, foliolis linearibus fasciculatis.* Lin. Sp. Pl. 260.
Oenanthe striata rigida. Hort. Cliff. 99.
- 1888 *Silene floribus fasciculatis, calycibus tomentosis.* Fl. Virg.
- 1889 *Silene foliis quaternis.* Flor. Virg.
Lychnis Caryophylleus Virginianus, Gentianæ foliis glabris quatuor ex singulis geniculis caulem amplexantibus, flore amplo fimbriato. Raii Hist. tom. ii. p. 1895.
- 1890 *Siler montanum, foliis Aquilegiæ.* Mor. Hist. 3. 276.
Angelica montana perennis, Aquilegiæ folio. Tourn. 313.
- 1891 *Sisyrinchium foliis gladiolatis amplexicaulibus pedunculis brevioribus.* Dict. Hort.
Bermudiana iridis folio fibrosa radice. T. Inst. 338.

1892 Soli-

- 1892 *Solidago paniculata*, racemis recurvatis, floribus erectis, foliis lanceolatis integerrimis glabris. *Miller's Icons*.
- 1893 *Sorbus Sylvestris* foliis domesticæ similis. C. B. P. 415.
Sylvestris five *Fraxinus bubula*. Ger. 1290.
- 1894 *Styrax*; folio *Mali Cotonei*. C. B. 452. *Officin.* 473.
- 1895 *Thapsia montana omnium maxima*, foliis lobatis, umbellis luteis. *Micheli Hort. Flor.*
- 1896 *Tithymalus arboreus*. *Alpini*.
- 1897 *Tithymalus Cyparissias*. *Alpini Exotic*.
- 1898 *Veronica*; flosculis oblongis pediculis infidentibus, *Chamædryos* folio. *Hist. Ox.* 2. 322.
- 1899 *Viburnum Mathioli*. C. B. 429.
- 1900 *Vitex* foliis ternatis quinatisque pinnato incisis, spicis verticillatis terminalibus. *Dict. Hort.*
-

LIX. *Some Account of the Animal sent from the East Indies, by General Clive, to his Royal Highness the Duke of Cumberland, which is now in the Tower of London: In a Letter from James Parsons, M. D. F. R. S. to the Rev. Tho. Birch, D. D. Secretary to the Royal Society.*

Read March 27,
 1760.

AT the request of the Reverend Dr. Littleton, dean of Exeter, I went to observe this creature, in order to find what class of animals he belonged to; and made the drawing,





IParsons MD. ad vivum del. mart 25 1760.

J. Mynde. Sc.

ing, now before the Royal Society, for its inspection. [See *Tab.* XIV.] I have endeavoured to make it as accurate as possible in all its proportion; yet am afraid I have made the ears a small matter too long. There is a figure of it in the London Magazine for December last, which has no resemblance at all to it, except in the ears, which the engraver, who drew it, has made to turn forwards, contrary to nature. However, the following description will, I believe, be the proper account of it.

It is something taller than the largest sized cat, being about fifteen inches high at the shoulders; slender and light, though strong. The head is small in proportion to the rest, and the neck slender. It has nothing fierce in its aspect, but is mild, and very tame. It is exactly of a fawn-colour, having its ears black on their outsides, and lined with white hairs, and some white round the root of each ear; it is also white under the throat and belly, and a little so on the backs of its limbs. Its eyes are small, and its head like that of a cat, but somewhat slenderer; its legs are genteel and strait, with the paws of a cat, having the power of dilating and contracting its toes, which are armed with strong crooked nails, in the same manner as a cat or tyger does; and its actions are like those of a cat. I sat and watched its motions, and saw it lick its foot, and rub it over its face several times, exactly like a cat; and was told by the man who shewed it me, that, if it is offended, it hisses. I examined its teeth, and find them in the same number and manner with those of a cat. And as to its food, they give it raw mutton every day; and when it is sick, which it often is, they give it a
live

live fowl, or rabbit, which it seizes eagerly, and lies upon it without motion, for a considerable time, to suck the blood, and this proves a certain cure. The figure shews it to have also a tail like that of a cat.

None of the natural historians have any account of this animal, that I have yet seen, except the learned Dr. Walter Charleton *, who has a bad figure of it, engraved at the expence of Dr. John Lawson, his cotemporary, of the college of physicians, as it appears in an inscription at the bottom of the plate, wherein the head is, contrary to truth, very large and strong in appearance, the tail like that of a fox, and the whole as strong as a mastiff dog: the name given it in the plate is the same with this, but differently spelled, thus, *Siyah-ghush*.

This author very justly ranks it among the cats, and has given such an account of this animal, as well deserves the notice of this learned Society, of which I have made the following English extract.

“ Among the wild cats, which vary according to
 “ the difference of climate, manner of living, and
 “ the like, none is more worthy of notice than that
 “ which is now kept in the park of our sovereign
 “ King Charles II. It was sent to the king by an
 “ English gentleman, who was governor of our mer-
 “ cantile affairs in the dominion of Surat, and is called,
 “ among other names, in the Persian language, *Siyah-*

* See his *Exercitationes de Differentiis et Nominibus Animalium*.

“ *ghush*, that is, *Black-ear* *, all along the coast of
 “ Coromandel, and indeed all over India. It is about
 “ the size of a fox, but like a cat in its form; and
 “ has the cunning and cruelty of a leopard, with
 “ the limbs of a cat, but longer and stronger; having
 “ so much strength, that I saw it kill a hound, that
 “ came in its way, in a moment. The legs are
 “ thick set with hair, and its nails concealed under
 “ them, which are never extended but upon seizing
 “ its prey, which is common to lions, panthers, and
 “ domestic cats. But what seems peculiar to this
 “ animal is, that, having jumped upon his prey, he
 “ lies upon it unmoved, holding his bite, as if he
 “ was dead, whether for joy, or in order to drink the
 “ blood of the creature. The great men in India
 “ have them bred up tame, because of their dex-
 “ terity in catching birds, hares, rabbits, and such-
 “ like; and such is their craft and fierceness, that
 “ they will seize even a fox; but their keepers will
 “ not suffer them to attack any thing above their
 “ strength, and therefore they only set them at cranes,
 “ geese, ducks, pheasants, partridges, peacocks, and
 “ such-like game, which they seize by many kinds
 “ of deceit, to the great pleasure of the spectators;
 “ and catch those timid animals, the hare, rabbit, fawn,
 “ goat, &c. by swift running, and sometimes by
 “ craft.

“ When they are sick (which, from over-gorging
 “ their stomachs, they often are) their keepers steep

* Dr. Charleton says, that he was obliged to his good friend the
 learned Dr. *Thomas Hyde*, then principal librarian to the Bodleian
 library, for this interpretation, who was well versed in the oriental
 languages.

“ a piece of tender meat in human urine, and feed
 “ them with it, and being bruised or tired by over-
 “ hunting, they give them some *Mummy*, wrapped up
 “ in their meat, and a warm place to rest in till they
 “ recover.”

It is said of this animal, that he follows the lion at a distance, in order to feed on what he leaves of the animals he destroys. To illustrate this, Dr. Charleton quotes a passage from the *Apolog.* of *Sheich Saadi*, which was written five hundred years before, and published in Persian and Latin by *Georgius Gentius*; which is in English as follows :

—This *Black-Ear* is asked, “ What makes him
 “ keep company with the lion, and seems so officious
 “ to please him ?” to which he answers, “ That I
 “ may feed on his leavings, and lead my life safely
 “ under his protection.” To which it is replied,
 “ Since you live under the shadow of his protection,
 “ and draw such benefit from him, why do not you
 “ approach nearer to him ?” He answers, “ If I took
 “ your counsel, and come near to him, I should not
 “ be safe from his fury a moment.”

Now from this particular account by the learned Charleton, and my own observations on him and his actions, I am inclined to rank this animal among the cats; and join with *Linnæus*, who, in his *Ordo secund.* has a fifth species of *Felis*, which agrees well with the principal characters of the animal before us: his words are,

“ *Felis cauda elongata, auribus——*
 “ *—————penicilliformibus.*”

LX. *An Account of the Frog-fish of Surinam, addressed to the Royal Society, by Mr. George Edwards, F. R. S.*

Read March 27, 1760. **I** Have now the honour to lay before this Royal Society an animal not to be found in the British Museum, nor in any other collection that I have seen in England, and which, perhaps, deserves attention, in regard to what is said of its strange metamorphoses, as much as any part of natural history whatever. It was brought from Surinam in South America, by the way of Barbadoes, to John Fothergill, M. D. of London, and is the animal, which Merian and Seba describe as changing from a frog into a fish.

In the appendix to Merian's Nat. History of the Insects of Surinam, where she treats of the transformation of fishes into frogs, and of frogs into fishes, after explaining, how the European frog is changed from a minute fish (or tadpole *) into a perfect frog, she proceeds to describe the gradual transformation of a species of frogs found in great numbers in the river of Surinam, into perfect fishes, and gives five figures to illustrate her description; the subjects whereof, she says, were then in the collection of Albert Seba at Amsterdam, from whom she also had her

* I have grossly copied Mrs. Merian's five figures from Plate LXXI, the better to explain her descriptions, which figures are herewith presented. Linnæus calls this animal *Paradoxa*, Last Edit. of his *Systema Naturæ*, p. 212.

figures and information, as appears since by the account published by Mr. Seba of his curious cabinet of natural history, in two pompous folio volumes, a copy of which, finely illuminated, is now in the British Museum.

Mrs. Merian's Description is as follows.

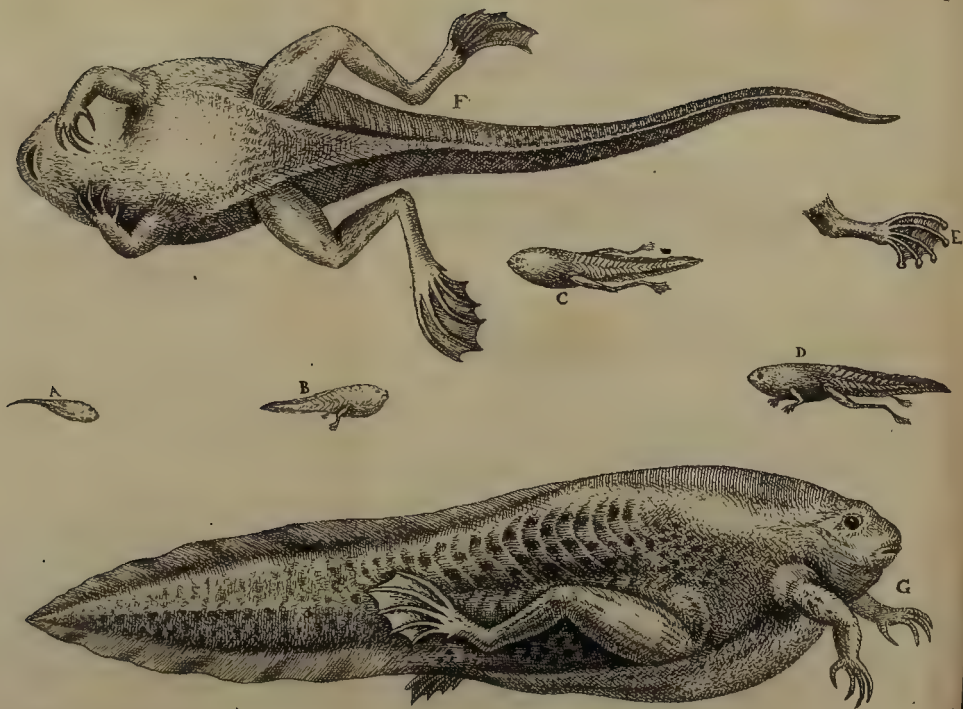
“ The first figure shews the perfect frog, brown, yellow, and green, in spots, but paler on the belly, the hinder feet webbed like those of a goose, the fore feet without webs : the second figure represents the first change the frog undergoes, by the growing out of a tail ; after which it gradually acquires the shape of a fish, the two fore feet decreasing and perishing by degrees, as is shewn by figure 3. also the decrease of the hinder legs, as is shewn by figure 4. and, lastly, the frog is changed to a perfect fish, as at figure 5.

“ The Indians and Europeans of Surinam call these fishes Jakies; they are cartilaginous, of a substance like our *Mustela* †, and very delicate food. A bone (or cartilage) runs down the back, with small bones all over the body, which is divided into equal parts; they are adorned with tender beautiful scales, are first of a darkish colour, and afterwards grey.”

The frogs of Asia and Africa are described by this author, plate 72. to change gradually from fishes into frogs, as the European frogs do ; but then she tells

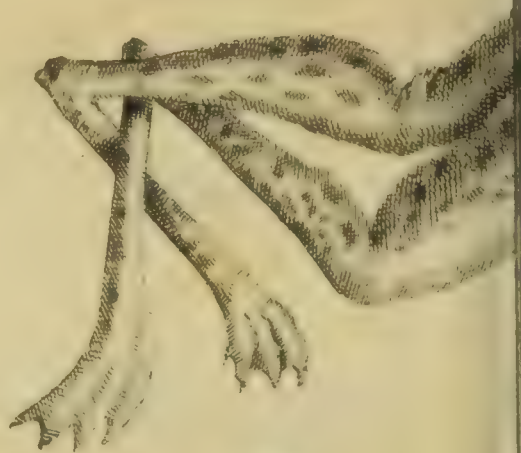
† A fish of the cartilaginous kind : See Willoughby de Piscibus, Tab. B. 5. and Edwards's Gleanings, Tab. 288.





The Frogfish, of Surinam Dragon from nature of the size of Life ~~~~~ George Edwards. Delin: et Sculp: Mar.







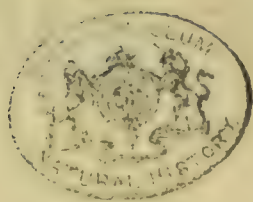
I



III



IV



us that, after many years growth, they change again into fishes, tho' the manner of their change is unknown.

The natural subject before us, two views of which I have given in a print annexed, see Tab. XV. Letters F G. They are drawn and engraved immediately from nature, of the size of life, which, on comparison, I find to be more than double the size of what Merian has figured in her book, plate 71. which inclines me to think this before us to be specifically different from Merian's: her figure expresses the fin, which passes round the tail, scalloped, whereas in this before us the edge of the fin is perfectly even, without any indentures: the hinder feet in her figures have only four toes each, whereas this before us hath five, webbed together, besides a small rudiment of a toe, as may be seen in the natural subject, as well as in my print. The fore feet, both in Merian's and my figures, express four toes, unwebbed, tho' I find the four toes to stand, three of them in the form of fingers, and one opposite to them, which serves as a thumb, which is not expressed in Merian's figure. The general shape of it is expressed by the figures F. G, in my print. The skin is soft, without any thing of scaliness, and the fin round the tail is like thin wet leather, without any fibres to support it, as is common to most fishes. The toes in all the feet end in points, but are without nails or claws. On piercing the tail, or hinder part, with a needle, I found no bony resistance; but on piercing the thighs and head the bones strongly resisted. Its colour in the spirits appeared dark brown, with dusky spots, the under side lighter, and without spots,

There

There came in the same glass of spirits with the above-described frog-fish (as I have chose to call it), a parcel of small tadpoles, in their different changes from the spawn to near the perfect frog : see them expressed on the plate of their natural size at letter A B C D. Doubtless these produce a species of frogs different from the European, and perhaps are the same that Merian and Seba describe as changing into fishes. I think, however, that our assent to such an opinion may reasonably be suspended, till we are confirmed in it by farther observations of the real fact ; for it seems strange, that a tadpole should first be changed into a frog, and that the self-same frog, by a reversed process of nature, should change again into a very large tadpole, as figured at G in the plate, and finally change into a fish, as in Merian, tab. 71. fig. 5. It seems very strange, that another tail should grow from the frog that hath lately lost one, and that he should gradually lose his legs, and become a perfect fish. Nature, in her ordinary course, is not accustomed to act in such a manner backwards and forwards, to seem to perfect a work, and then to reverse it by a process directly opposite.

The little tadpoles on the plate A B C D, are specifically different from the large one F G, as is manifest by the difference of their feet : see the hinder foot of one of the small ones magnified at E, which shews, that the ends of their toes are round and flat on their undersides, both in the hinder and fore feet : forwards they have four toes in each foot, unwebbed ; the hinder feet have five toes, each webbed together.

As to the specimen before us, whether it is already in its perfect state, a species of frog having a tail, or a
kind

kind of water-lizard, I shall not pretend to determine : but, considering its size, if it should be deemed a tadpole, as first produced from a spawn, and in its progress towards a frog, such a frog, when full-grown, if it bears the same proportion to its tadpole that the frogs in Europe do, it must be of an enormous size ; for our full-grown frogs exceed the tadpoles, and young fry of perfect frogs, at least fifty times in magnitude.

London, March 27, 1760.

LXI. *An Account of a remarkable Operation on a broken Arm; by Mr. Charles White, Surgeon at Manchester, communicated by George Lloyd, Esq; F. R. S.*

Read March 27, 1760. RObert Elliot, of Eyam, in Derbyshire, a very healthful boy of nine years old, had the misfortune, about midsummer in the year 1759, by a fall, to fracture the *Humerus*, near the middle of the bone. He was immediately taken to a bone-setter in that neighbourhood, who applied a bandage and splints to his arm, and treated him as properly, as, I suppose, he was capable of, for two or three months. His endeavours, however, were by no means productive of the desired effect, the bones not being at all united. A surgeon of eminence in Bakewell was afterwards called in ; but as he soon found he could be of no service to him, and, as the case was very curious, he advised the lad's friends

friends to send him to the infirmary at Manchester : he was accordingly brought thither the Christmas following, and admitted an in-patient. Upon examination, we found it to have been a simple oblique fracture, and that the ends of the bone rode over each other. His arm was become not only entirely useless, but even a burthen to him, and not likely to be otherwise, as there was little probability, that it would ever unite, it being now near six months since the accident happened.

Amputation was therefore proposed as the only method of relief ; but I could not give my consent to that ; for, as the boy was young, and had a good constitution, it was hardly possible that it could be owing to any fault in the solids or fluids, but that either nature was disappointed in her work by frequent friction, while the callus was forming, or rather that the oblique ends of the bone being sharp, had divided a part of a muscle, and some portion of it had probably insinuated itself betwixt the two ends of the bone, preventing their union. Which ever of these might be the case, I was of opinion, that he might be relieved by the following operation ; *viz.* to make a longitudinal incision down to the bone, to bring out one of the ends of it (which might be done with great ease, as the arm was very flexible), and cut off the oblique end, either by the saw or cutting pincers, then to bring out the other end of the bone, and cut off that likewise ; afterwards to replace them end to end, and then treat it intirely as a compound fracture.

The objections, made by the other gentlemen concerned to this proposal were, 1st, The danger of
wounding

wounding the humeral artery by the knife; 2d, the laceration of the artery by bringing out the ends of the bones; and, 3d, that we had no authority for such an operation. As to the first, that was easily obviated, by making the incision on the side of the arm opposite to the humeral artery. The place of election appeared to me, to be at the external and lower edge of the deltoid muscle, as the fracture was very near to the insertion of that muscle into the humerus, the danger of wounding the vessel not only being by that means avoided, but after the operation, while the patient was confined to his bed, the matter would be prevented from lodging, and the wound be easily come at, to renew the dressings. The second objection will not appear to be very great, when we consider, that in compound fractures, the bone is frequently thrust with great violence through the integuments, and seldom attended with the laceration of any considerable artery, and as this would be done with great care and caution, that danger would appear very trifling. The third and last Objection is no more than a general one to all improvements.

This method, which I have been proposing, was at last resolved upon, and I assisted in the operation, which was performed by a gentleman of great abilities in his profession, on the third of January, in the present year. The patient did not lose above a spoonful of blood in the operation, though the tourniquet was not made use of. When the operation and dressings were finished, the limb was placed in a fracture-box contrived on purpose, the lad confined to his bed, and the rest of the treatment nothing different from that of a compound fracture.

The wound was nearly healed in a fortnight's time, when an erisipelas came on, and spread itself all over the arm, attended with some degree of swelling: this by fomentations and the antiphlogistic method soon went off, and the cure proceeded happily without any other interruption. In about six weeks after the operation, the callus began to form, and is now grown quite firm: that arm is as long as the other, but somewhat smaller by such long continued bandage; he daily acquires strength in it, and will soon be fit to be discharged.

C. White,

Manchester,
March 17, 1760.

*Surgeon to the Infirmary
at Manchester.*

LXII. *An Account of a Bone found in the Pelvis of a Man at Brussels; by Terence Brady, M. D. In a Letter to the Rev. Tho. Birch, D. D. Secretary to the Royal Society.*

Rev^d. Sir,

Read Apr. 29, 1760. **I** Send you here inclosed a draught of a bone found in the pelvis or basin of a man, that died in the military hospital of this town the 12th of March 1760, of a seven days inflammatory distemper. This extraordinary concretion weighs about 20 ounces, has all the external appearances of a bone, with the hardness, solidity, and specifick gravity





vity of common stone. It is chequered or marbled, as you see in the figure, *Tab. XVI.* by which you can distinguish the primitive particles of the bone whiter and harder than the darker part: it was formed on the lower extremity of the mesocolon, and probably as it grew big was carried down by its own weight into the bason, where it had no adherence to, nor connection with, any of the adjacent parts, but lay in its own very thin membrane or periostium, betwixt the os pubis and the bladder, somewhat to the right side. It was joined to the mesenterium by a tough, compact, membranaceous, glandulous substance, in the form of an inverted cone, whose point was firmly inserted in the cavity observed on the top: here the membranaceous fibres were turned into bones, or, *vice versa*, the fibres of bones degenerated into fleshy membranes: there was no intermediate cartilaginous substance to be observed. By drawing up this conic body with very little effort, the bone followed, to the great surprise of all the spectators; after which there was no farther inquiry made, in regard of the other viscera of the abdomen. It was only taken notice of, that the omentum was quite consumed, and the mesenterium very much swelled and scirrous.

The man, that bred this monstrous bone, was called Straßzet, was born in Bavaria, and lived to the age of 55, of which he was 28 years a musketeer in Ba-reith's imperial regiment. He was always strong and healthy till about five years ago, when he began to complain of the hardness of his belly, and now and then of a suppression of urine, of which last inconvenience he could help himself, by turning on his right side, and lying a little on his face: by that po-

sition, the stone did no more press on his bladder. He never missed doing soldier's duty till his last sickness, about seven days before his death.

We have examples of membranes, and of several soft parts of the body, being ossified; but I believe there is not such a monstrous production as this to be seen any-where.

About 20 years ago I saw at Mantua, two inches of the aorta near the heart turned to bone, in a man that was a long time tormented with a violent palpitation of heart.

I know, Sir, relations of such extraordinary appearances are often, and with reason, suspected of exaggeration, but you may depend upon the veracity of this. I am,

Reverend Sir,

Your most humble, and

most obedient Servant.

Brussels, March 29,
1760.

Terence Brady, *M. D.*

LXIII. *An Account of an extraordinary Case of a Lady, who swallowed Euphorbium. By Dr. Willis, of Lincoln: Communicated by Sir Francis Dashwood, Bart.*

Read Apr. 24, 1760. **I**N December 1758, Mrs. Willis of Lincoln fell into a slow fever, occasioned by too small a discharge of the lochia after lying-in,

lying-in, and a redundancy of milk, the consequence of her not suckling her child. On the 18th day after her delivery, by the mistake of her nurse, she took, instead of a draught that was ordered for her, two ounces of the tincture of euphorbium *. The shocking symptoms, which immediately ensued, violent suffocation, and an intolerable burning pain in the mouth, throat, and stomach, soon discovered the horrible mistake. I was in the room in about four or five minutes after the accident happened, unapprised of the nature of it, and therefore the more shocked, when I found every body in tears of despair, offering at no means of relief, as they had no hopes of success.

As soon as I was made acquainted with what had happened, it occurred to me, that warm water and oil were the likeliest things to correct and expel the poison. I imagined a large quantity of warm water might probably make the patient vomit, and in some measure help to discharge the caustic tincture. I was sure the water would at the same time mitigate its violence, by diluting it; and by precipitating the acrid gum from the spirit, whereby it would necessarily be hindered from touching the membranes of the stomach and bowels in so many points, and from penetrating into their substance.

There was happily a large tea-kettle of water on the fire, of which, being first qualified with a proper

* The tincture was thus made ;

R *Gum. Euphorb.* ʒij. *Spt. Vin. rectif.* ʒij. *Sol. add. Camph.* ʒij. The camphor was ordered to weaken the caustic quality of the tincture, which being applied to a horse's leg without the camphor had made a blemish.

quantity of cold water, I immediately gave the patient a basin lukewarm, and repeated it as fast as possible, conjuring her to use her utmost resolution to swallow; which she certainly did in a most surprising manner. After the third basin, she vomited very freely: what was brought up smelt very strong of the camphor, and seemed to contain a good deal of the tincture, with the gum separated from the spirit. She still drank on, but complained of excessive burning and torture in her stomach, crying out continually, she was burnt to death.

I had then recourse to oil between whiles, in the quantity of two or three ounces at a time; and drenched her plentifully sometimes with oil and sometimes with water. She vomited very copiously, and I repeated the oil and water interchangeably, till she had taken, as well as I could guess by the vessels, two gallons of water and a flask of oil in a very short time †.

Imagining

† Dr. Sydenham, being called to a man, who had taken *Mercur. sublim. corros.* about an hour before the doctor saw him, the poison having affected his lips, &c. only ordered water to be taken in a large quantity, and thrown up copiously in glisters. But as the corrosive sublimate of mercury is to be considered as a poison, whose caustic acrimony consists in a saline principle, and water is the proper solvent, diluent, and vehicle of all saline substances, the propriety of Sydenham's ordering water alone is sufficiently apparent. Poisons of a saline nature being dissolved in the fluids of the stomach and intestines, do not confine their ravages to these parts only, but are apt to enter the absorbent vessels, and insinuate themselves into the road of the circulation. Water is here a good antidote, as it dilutes such substances, washes them off the sensible membranes, destroys their acrimony, and readily passing through all sorts of canals,

seen

Imagining the deleterious draught had not had sufficient time to bring on any violent inflammation or excoriation, or to make its way into the blood, vomiting and purging, with plenty of diluents and sheathing substances, seemed the likeliest means to save the patient, if any thing could be hoped to succeed in so perilous a situation. I therefore ordered a mild but operative emetic of *Pulv. Rad. Ipecacuan.* and a mixture with *Sperm. Ceti* and oil to be taken occasionally; still following up the patient with oil and water. I had reason to expect the emetic would also purge as well as vomit, and not only clear the stomach of the remains of the poisonous draught, but likewise carry downwards what portion of it might have passed through the pylorus by the contraction and agitation of the ventricle on the preceding vomiting. The apothecary demurred at the emetic, and

soon carries them out of the body. But the case is otherwise with gummy resinous poisons, such as euphorbium. These being indissoluble in water, are not so apt to enter the absorbent vessels, and pass into the blood, but, by their acrimony, shut up the orifices of those canals, and preclude a passage. Therefore oil here should be called in to the assistance of water. For the caustic resinous substance of euphorbium being precipitated or separated from the spirit, and formed into clots by the water, would still be apt to stick to the tender nervous membranes of the stomach and bowels, and by its intolerable acrimony cause violent vellications, inflammations, and gangrene. But the oil contributes greatly to prevent these fatal effects, by sheathing the corrosive acrimony of the poison, preventing its adhesion to the delicate lining membranes of these first passages, and defending them from the violence of its attacks; while at the same time it promotes the discharge of their contents.

These qualities of oil also render it very serviceable in other species of poison.

objected

objected the danger of its aggravating the effects of the poison by its stimulus and irritation. Though I was not in the least convinced by the objection, yet, from an apprehension of the reflections, which might probably be made after the tragical scene, which seemed to be inevitable, I was staggered in my proceeding, and wished the objection had not been started. My brother, observing my uneasiness, asked me, if she should send for Dr. Dymock. I gladly accepted this offer, as it rid me of my perplexity, and would give satisfaction to all concerned to have had the best advice. In the mean time, I plied the patient with oil and water alternately, with which she vomited; but still grievously complained of a burning heat in the stomach and bowels. Her breath and all she vomited smelt very strong of the camphor. Her pulse was moderate, and not much quickened. I had now given her about a gallon more of water, and half a flask more of oil, when Dr. Dymock arrived. Upon informing him of all the particulars of the case, he, without hesitation, ordered an emetic of *Vin. Ipecacuan.* ℥. which was immediately given: but the patient complained more and more of an inward burning heat, which made it necessary to supply her with more water before the emetic operated. It had however in a short time the desired effect, and operated plentifully both by vomit and stool, especially the latter way. The stools, which continued to be discharged for near an hour, without any griping pain, very manifestly discovered both camphor and oil being mixed with them. The purging now began gradually to abate, and soon after the burning heat in the region of the stomach became
more

more tolerable, and insensibly grew better, the camphor being no longer perceptible in the breath or evacuations. Her drink now was water with the addition of a little milk.

The patient's spirits, which, by means of the great irritation and feverish tumult the caustic tincture had excited, kept up surprisngly, now began to fail her, and she was with difficulty got into bed: where, after complaining for a short time only of a soreness in the first passages, she lay sweating profusely for four hours in a very low desponding condition.

A gentle opiate was exhibited, which took effect; and after a sound sleep of five hours the patient waked very easy, took some of the sperma ceti mixture, and had another sleep of three hours. She now found herself free from all her complaints; the previous slow fever, as well as the effects of the poisonous tincture, being entirely carried off.

It is remarkable, that the patient found herself, for four days successively, in so happy a state of ease and tranquillity, as she had never before experienced, and to this day enjoys a perfect state of health.

Thus have I laid before you the true history of this case; for the simple relation of which I flatter myself I need make no apology, when I reflect, that the practice of physic must derive much greater advantages from plain histories of matter of fact, and just deductions drawn from them, than from the most ingenious hypotheses and speculative theories. These were of baneful influence to the progress of true knowlege, till your illustrious Society taught the world the true method of investigating the laws of

nature, by observation of fact, experiment, and rational deduction.

The following observations, which this case naturally suggests, seem to deserve our attention.

1st, That in any similar accidents of swallowing corrosive poisonous substances, a quick and resolute administration of these simple bodies, water and oil, in a large quantity, seems to be the most effectual method of preventing any bad consequences, and far preferable to the numerous boasted antidotes, which have been handed down to us.

2^{dly}, That an emetic may be more safely and effectually administered, and its operation waited for, after the acrimony of the poison hath been sheathed and blunted, and the coats of the stomach defended from its attacks, by a liberal use of water and oil, than immediately after it is swallowed.

3^{dly}, That as the slow fever and redundancy of milk, as well as the poison, were carried off by the copious discharge excited in the easy manner above-mentioned; might we not often hope for success in fevers occasioned by similar causes, plenitude and obstruction, from plentiful evacuations, brought on after the same manner, by simple, diluent, and sheathing medicines?

4^{thly}, The camphor was undoubtedly of great service in curbing the destructive effects of the euphorbium, by blunting its acrimony, and soothing the nerves into an insensibility of irritation, and consequently an incapacity of spasmodic affections. I have tried the *tinēt. euphorb. cum camphorâ* on a horse's

horse's leg several times, and find it not near so caustic as without the camphor. And it is well known how much camphor involves the spicula, corrects the acrimony, and mitigates the effects of cantharides, saccharum Saturni, and rough, mercurial, and antimonial preparations.

5^{thly}, To water and oil therefore we may justly add camphor as a powerful corrector and expeller of poisons in general. This it probably effectuates, 1st, by blunting the acrimony; 2^{dly}, by calming the nervous system, and securing it from spasmodic tumult and convulsion, which may be a consequence of its sheathing quality; 3^{dly}, by its extreme subtilty and volatility, whereby it freely penetrates the smallest recesses of the body, and powerfully promotes a diaphoresis. Some late instances of the effects of camphor in poisonous cases greatly confirm this account.

These three simple bodies then, water, oil, and camphor, challenge the first place among the antidotes hitherto discovered, both for internal and external use, and are much more to be depended upon than any of the elaborate compositions calculated for this purpose by the ancients, as the *Theriac. Androm. Mithridat. Conf. Paulin. &c.*

LXIV. *An Account of artificial Cold produced at Petersburg : By Dr. Himfel. In a Letter to Dr. De Castro, F. R. S. Translated from the French by James Parsons, M. D. F. R. S.*

Read May 1. 1760. ON the 14th of December [1759] we had, at Petersburg, the most excessive cold weather that ever was known, even to 205 degrees of De Lisle's thermometer. At that time Professor Braun repeated Fahrenheit's experiments, in order to produce excessive cold by means of spirit of nitre combined with snow. He saw, with surprise, the quicksilver fall considerably in the thermometer, and descend even to 470 degrees at last : there the quicksilver remained fixed in the open air for the space of a quarter of an hour, and did not begin to rise till it was carried into a warm room. He repeated the same experiment, first with the same, and then with another thermometer, with the same success. The immobility of the quicksilver made him conjecture, that it might be frozen, or become a solid body. But as Mr. Braun had not broken the glasses, he could only at that time form a conjecture. On the 17th he produced, again, cold equal to that of the 14th ; but on that day there was no experiment made ; and Mr. Braun communicated his discovery at a meeting of the academy. On the 25th of December in the morning, between nine and ten, De Lisle's thermometer was at the 199th degree of cold ; and Mr. Braun, as well as Professor Æpinus,

Æpinus, then repeated this experiment. As soon as the former had observed the quicksilver immoveable in the thermometer, he broke the glass; and he found, to his amazement, the quicksilver frozen, but not intirely; for in the middle of the glass ball there was a small portion yet remaining fluid. Mr. Æpinus's thermometer fell, with extreme rapidity, almost to the 500th degree, and in breaking the glass from below, he found the quicksilver contained in it absolutely frozen. Both the gentlemen found, that the quicksilver, thus rendered solid, bore hammering and extension, like other metals; but being afterwards exposed to the open air, it recovered its former fluidity in a little time.

Mr. Æpinus went somewhat farther, in order to examine the quicksilver, when it was made solid. He poured quicksilver into a glass tube as thick as one's finger, closed at bottom, but open at top. The quicksilver in this cylinder, which was about one inch and half long, froze in three quarters of a minute; and he observed, that it became solid, perfectly resembling other metals, except iron: it continually contracted, and its surface, which was at first pretty high, soon sunk very low. This cylinder of frozen quicksilver sunk to the bottom of fluid quicksilver, in the same manner, as is observed of other metals, except iron. We know the contrary happens with regard to water frozen and other fluids, which extend as they become solid, and their ice swims in the fluid matter, of which they were produced.

On the 26th of December in the morning, between nine and ten, the cold became extremely sharp at 211 degrees, and such as exceeded the greatest
 3 degree

degree of artificial cold fixed by Fahrenheit; for 40 degrees below *zero*, in Fahrenheit's thermometer, is equal to 210 degrees of that of De Lisle.

Mr. Braun repeated this experiment again, exactly with the same success with that of the day before. The counsellor and professor Lomonossov made the same experiment on the same day; and by means of aqua fortis the cold came to 495 degrees. He then poured in spirit of common or sea salt, and the quicksilver fell down in the thermometer to 554 degrees; and in taking the thermometer from the mixture, the quicksilver continued to fall in the open air to the 552d degree. He threw yet into the glass a little more snow, pouring on it some oil of vitriol, and suddenly the quicksilver fell to 1260 degrees. Then he broke the ball, and found the mercury changed to a solid body. The quicksilver, which yet remained in the tube, was also become solid, and appeared like a loose silver wire, attached to the ball, which was flexible every way. He gave the ball of quicksilver several blows with a turned ax, and it became flat like a half-ruble, or English half-crown; but receiving thereby some cracks, it dissolved in about 20 minutes. These experiments were made when the air was at about 208 degrees of cold.

Mr. Kraze, Mr. Zeicher, and the first apothecary Model, and again Mr. Æpinus, repeated the same experiment with the very same success. It is to be observed, that at the second experiment by professor Zeicher, on the 31st of December, as the air was then only at the 183d degree of cold, in taking the thermometer from the mixture, in which the quicksilver was at 300 degrees, it fell yet 100 de-
grees

degrees more in the open air. The testimony of so many philosophers, each of whom had respectively made the experiment, will, no doubt, be sufficient to prove the truth of it. But in order to remove all doubt about it, it must be remarked, that distilled quicksilver only was made use of in every experiment; nay, in some, the quicksilver was revived from sublimation. There can therefore be no suspicion, that what they used was impure, or mixed with any heterogeneous matter. This appears to have happened to Mr. De Lisle de la Croyere, when he says, that in Siberia he found the quicksilver congealed in the barometer: and even his papers, which are in the academy, shew that he made a mistake in his remarks; for, according to them, the mercury became solid as soon as it fell to about 195 or 200 degrees: but the mercury, which is pure, does not congeal at that degree; for otherwise it would not be very extraordinary with us to see it take a solid form, because it is not rare to find the cold at this degree here. We may believe, that the quicksilver used by Mr. De la Croyere was impure, and therefore might sooner become an amalgama than pure mercury.

Now there are two things we cannot reason upon with any certainty: As to the hardness of the quicksilver congealed, it appeared to have had, in some essays, less hardness than lead, and in others more: also we cannot be very sure of the degree of cold, by which the mercury is consolidated. The greatest part of the experiments agree in this, that the quicksilver becomes solid, when it falls in the thermometer to 500 degrees, more or less. Nevertheless, they do not so sufficiently agree as to deduce any thing certain about it.

Although

Although in the experiment made by Mr. Lomonosſow the quickſilver fell to 1260 degrees, this philoſopher nevertheless ſays, that he could not ſufficiently obſerve, in his hurry, whether the ball might not have received ſome crack, and the quickſilver thereby perhaps might have had liberty to fall the lower, which otherwiſe would not have happened; for the ſame thing happened to Meſſ. Braun, Zeicher, and Æpinus, that the balls of their thermometers were cracked and broken. By the experiment of Mr. Æpinus, made on the 25th of December, in which the quickſilver fell ſuddenly in the thermometer, and the cylinder of quickſilver of the thickneſs of one's finger becoming ſolid ſo quickly, it may be obſerved, that the degree of cold then produced ought to exceed 300 degrees. Nevertheless, whatever the degree be, we cannot determine how the common thermometer ceases to be of uſe as ſoon as the mercury begins to become ſolid.

Here follows an account of the manner, in which theſe experiments may be made, that other philoſophers may be capable of repeating them. It is therefore to be obſerved, that it is neceſſary to uſe fuming ſpirit of nitre, or of ſuch as is evaporated till the fumes become red; for the common aqua fortis, which is uſed, had not the deſired effect. Mr. Æpinus has found, that this experiment is very eaſily and ſpeedily made in the following manner. Take ſpirit of nitre, cooled as much as poſſible, and with it half fill a wine-glaſs, throwing in as much ſnow at the ſame time, and ſtirring it till it becomes of the conſiſtence of pap: then you have almoſt in an inſtant the neceſſary degree for the congelation of the quickſilver.

quicksilver: not only Mr. *Æpinus* used this method, but also Mess. *Krafe*, *Zeicher*, and *Model*, who followed Mr. *Æpinus*, and found it the most convenient method. Now, in reflecting upon the procedure of other philosophers, especially of Mess. *Muschenbroek* and *Reaumur*, for producing artificial cold, by the commixtion of snow with aquafortis, as the former has mentioned in his edition of the experiments of the academy of Florence, tom. i. p. 174. and Mr. *Reaumur*, in the memoirs of the academy of sciences of Paris for the year 1734. it is astonishing how it happens, that these learned men have not obtained, by a great deal, the degree of cold produced by the gentlemen of the academy of Petersburg; for their manner of making the experiments does not seem to differ much from that of Mr. *Braun*, as to what relates to any essential circumstances, nor from the manner mentioned before, so as to hinder them from producing effects nearly equal. Perhaps they may, in time, and by continued researches, be able to find out the circumstances, that prevented their success in the experiments of these great men: it may be, because the spirit of nitre was not endowed with its proper quality. In fine, I must further observe, that a certain degree of external cold appears absolutely necessary to the experiment. Mr. *Æpinus*, who made it the 28th of December, in a room where *De Lisle's* thermometer shewed 122 degrees of cold, cooled the spirit of nitre in liquifying snow to 150 degrees, and the snow, which they used, came to the same degree; in making the mixture, the result was an augmentation of cold to 300 degrees. It must then happen, that they had obtained the surprizing

degree necessary to congeal the mercury; which Mr. Zeicher also at length obtained; the degree of cold of the air being the 175th degree of De Lisle's thermometer, or the 30th of that of Fahrenheit.

LXV. *An Account of a complete Luxation of the Thigh Bone, in an adult Person, by external Violence; by Mr. Charles White, Surgeon, at Manchester. Communicated by George Lloyd, Esq; F. R. S.*

Read May 1,
1760.

AS Robert Hogg, (a farmer in Clyfton, about four miles from Manchester) a strong, robust, middle-aged man, was taking a load of wheat from off a horse, on the 20th of March 1759, his foot slipping, he fell backwards; his breech upon the pavement, and the load of wheat upon his belly and thighs. The servants carried him into the house, and laid him upon a bed, where he remained in the most racking torture, when I came to him, which was about two hours after the accident happened. I found his right buttock as large again as the other; the knee and foot of the same side turned inwards; and the thigh much shortened. Upon endeavouring to make the thigh perform its rotatory motion, there was not the least crackling to be heard. This convinced me, that the head of the bone was thrown out of the acetabulum; and, upon examination, I could distinctly feel it under the glutæi muscles: to which situation of it, and not to any bruise,

bruise, I was now satisfied that the size of the buttock was owing.

I soon reduced it, by the following easy, and very simple method.

Some napkins being first lapped round one of the posts at the foot of the bed, to prevent its galling him, I ordered the patient to be laid upon his back, with one leg on each side the post, and then directed three or four assistants to pull at the dislocated limb, the post now placed to his groin being a fixed point to pull against. Whilst they were making this extension, I clapped my left hand upon the head of the bone, to help it into its place; and, at the same instant, with my right hand, turning the knee outwards, threw the bone into the socket, with the greatest facility imaginable, but with such an uncommonly loud noise, as greatly astonished all who were present.

He was perfectly easy from that moment; the enlargement of the buttock entirely subsided. In a fortnight, he was able to move about, without assistance; and in two months afterwards walked as far as Manchester, being then quite sound; and the limb, that had been dislocated, of the same length with the other.

R E M A R K S.

Both antients and moderns have fallen into great errors, in regard to the treatment of accidents, that have happened to the hip joint. The antients, who, for want of frequent opportunities of dissecting bodies, were ignorant, that the neck of the femur was often broken, always imagined it to be luxated. Their

patients were, therefore, sometimes tormented, (in hopes of a reduction) without any advantage; and this want of success made the surgeons, at other times, abandon their patients, when they might have been relieved. The moderns have fallen into a contrary extreme, but attended with as bad consequences. Boerhaave, in particular, was of opinion, that there never was a dislocation of the thigh bone by any external violence, but that the head of it was commonly broken off at its neck, near the great trochanter. The opinion of so learned a man has had such weight with the generality of the profession, that it has been taken for granted, that, in these cases, the neck of the bone was always broken: consequently, the reduction was seldom attempted, and the unfortunate patients remained cripples during the rest of their lives. But the point is now, I think, cleared up, beyond the possibility of a doubt.

In the second volume of the memoirs of the royal academy of surgery at Paris, there are two cases related, to shew the resources of nature, where luxations of the thigh bone have not been reduced. Here it appears, (from examination after death) that, in the first case, the bone was thrown out upwards and outwards, the cotyloid cavity greatly diminished in size, and its figure changed from round to oval. The head of the femur was received into another cavity formed upon the os ileum, under the glutæus minimus, which served it as a capsula, to secure it within this preternatural cavity. This accident was occasioned by a fall, when the patient was a child. She was afterwards able to walk about, though she continued a cripple to the time of her death, which
happened

happened at the age of sixty-eight. In the other case, the bone was luxated downwards and inwards, and the head fixed upon the foramen ovale.

There is a case too related, and very well attested, in the Edinburgh essays, philosophical and literary, vol. 2d, of a man at Worcester, who had the head of the bone thrown out of the acetabulum, and lodged in the groin. It was with some difficulty reduced, and the man suffered no other inconvenience, than that of the leg's being about a quarter of an inch longer than the other.

To these, let me add, that, about thirty years ago, my father was sent for to a man, who had luxated his thigh bone three or four days before. The head of it lay in the groin, which the surgeon, who was first employed, did not discover. However, it was immediately replaced, and the patient recovered the use of his limb, in a very short time.

From what I have said, I would by no means have it concluded, that the neck of the bone is not sometimes broken, or that it is not even oftener broken than luxated: but, from the case, which has fallen directly under my notice, joined to those, which I have above recited, I think it must appear very clear, that it has been frequently luxated, and that two different ways.

C. White.

LXVI. *Conjectures upon an inedited Parthian Coin.* By the Rev. John Swinton, B. D. of Christ-Church, Oxon. F. R. S.

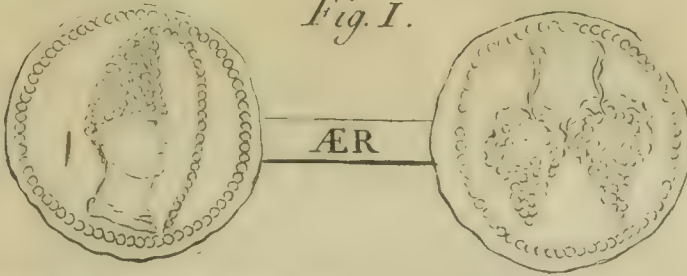
To the Rev. Thomas Birch, D. D. Secretary to the Royal Society.

Good Sir,

Read May 1,
1760.

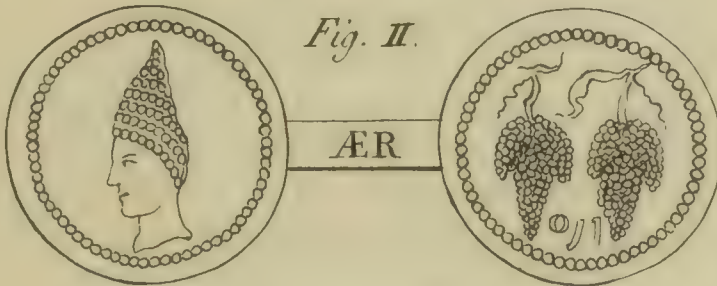
I Some time since met with a Parthian coin, nearly of the size of the middle Roman brass, that seems to merit the attention of the curious. For though the piece itself is in very indifferent conservation, yet it has not been hitherto published, and may perhaps serve to illustrate an exceeding rare coin, with a draught of which the learned world has been obliged by M. Bouterouë. As therefore it may possibly be deemed not altogether unworthy the notice of the Royal Society, I have taken the liberty to trouble them with a few conjectures upon it; especially, as they have done me the honour to order the publication of two papers, a few years since, upon similar subjects. To this I have likewise been farther excited by part of a letter, written lately to my worthy friend the Reverend Mr. John Nixon, a learned member of the Society, by Sig. Abate Venuti; who therein mentions the famous F. Corfini's approbation of my remarks upon Monneses's coin, inserted in the fiftieth volume of the *Philosophical Transactions*, excepting that he suspected the *Victory* on the reverse to allude to the Parthian monarch's devastation of the neighbouring provinces, and usurpation of the Parthian

Fig. I.



Apud Joannem Swinton, S.T.B. Oxoniens. R.S.S.

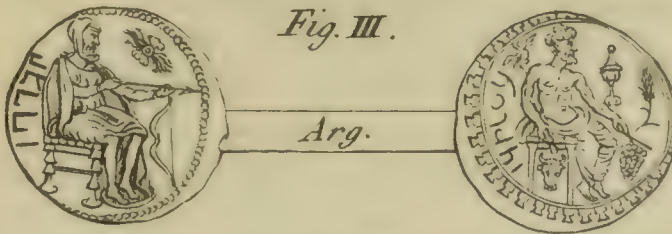
Fig. II.



1...2
J...I
O...y

Apud D. Bouteroue, p. 33.

Fig. III.



Apud Henricum March Armig.



Parthian throne. I have therefore endeavoured to remove the suspicion F. Corfini has entertained, and attempted to prove, that the impresson of this *Victory*, so utterly repugnant to the Parthian genius, must have been occasioned by some remarkable advantage, gained by the Parthian forces over the Romans. In order to which, I have at least in some measure evinced the Romans to have adorned with such a *Victory* the reverses of their coins, not only after a battle had been won, as F. Corfini would seem to maintain, but likewise on certain occasions after other prosperous military exploits. It is also here rendered probable, that the *Victory* was impressed on Monneses's medal, in allusion to the manner in which the Romans treated the Parthians, on their coins, after any favourable military event; that F. Corfini is not quite consistent with himself, the reason by him assigned for differing from me, on the principle he has advanced, being subversive of his own opinion; and that, as the reverses of the other Parthian medals never exhibit a *Victory* in the Roman taste, the coin before me could have been struck only to perpetuate the memory of some signal achievement, performed by the Parthian arms against the Romans.

If the conjectures now submitted to the consideration of the Royal Society should meet with the approbation of that most learned and illustrious body, they will undoubtedly be allowed to throw some light on the singular and valuable medal above-mentioned, published by (1) M. Bouterouë, never hi-

(1) *Recherches Curieuses des Monoyes de France &c. Par Claude Bouterouë, p. 33. A Paris, 1666.*

thereto explained. I therefore judged it not improper to transmit you an accurate draught of this coin, as well as one of the medal here so particularly described; [*Vide Tab. XVII.*] which may probably be the more acceptable to many, who have a taste for this kind of literature, as M. Bouterouë's book itself cannot now without great difficulty be found.

With regard to the language of the paper in which my remarks are contained, I shall only beg leave to hint, that it is understood by all who are proper judges of the performance. For this therefore I shall offer no other apology, than that the letter from Sig. Abate Venuti to Mr. Nixon, which occasioned it, is penned in the Latin tongue; and that the famous F. Corfini, the removal of whose doubt or suspicion was one of the principal objects I had in view, writes for the most part at least in the same language. I might however add, that many learned foreigners, who are particularly pleased with such disquisitions, are much better acquainted with Latin than any other tongue, except their own; and that it were to be wished the use of this noble language, in the republic of letters, were more general than it at present (2) seems to be. You will give me leave only farther to suggest, that there can be no impropriety in communicating to the Royal Society the following remarks, the (3) paper they are intended to support not only authorizing but even requiring such a com-

(2) *Nov. Act. Eruditor. Lips. Mensis Maii A. 1755. p. 252.*

(3) *Philosoph. Transact. Vol. L. p. 184—187.*

munication;

munication ; and to assure you that I am, with the
most perfect attachment and respect,

S I R,

Your most faithful,

Christ-Church, Oxon.

Apr. 24th, 1760.

and most obedient,

humble servant,

J. Swinton.

*In Nummum Parthicum hætenus ineditum
conjecturæ.*

I.

NUmisma planè incognitum, quod vulgavit (1) D.
Bouterouë, in adversâ parte regis cujusdam im-
berbis exhibet caput, tiarâ margaritis sive unionibus
fulgente ornatum ; in averâ verò binos racemos, ut
et trinas inusitatâ formâ literas, quarum potestas eti-
amnum ignoratur. Aliud item haud parùm detri-
menti à tempore perpeßum, cujus ectypum Regiæ
Societati jam visendum sisto, capite regis tali mitrâ

(1) *Recherches Curieuses des Monoyes de France &c. Par Claude Bouterouë*, p. 33. A Paris, 1666. Si *Gamma* pone *Vologesis* III. caput *Gazam*, vel *Gazacam*, *Mediæ Atropatiæ* urbem, indigitet, ut existimat *Vaillantius* (1) ; trina elementa incognita, in nummo *Bouteriano* conspicua, eandem forsan urbem designent. Quod et ipsæ eorundem formæ suadere videntur. Primum enim *Gimel* *Phœnicium* figurâ refert, secundum *Zain* *Chaldaico* et *Palmyreno* non est abfimile, tertium verò ipsissimum *Phœnicium* *Ain* se faciliè fatetur. *Ain* autem (2) nunc *Aleph*, nunc *Gimel* (3) sonuit. Hinc igitur vox *Gaza*, *Straboni* (4) usitata, *Gazag*, vel *Gazaca*, quæ *Stephano* (5) *Byzantino* magis placuit, fortasse sit enata.

(1) J. F. Vaill. *Asfacid. Imper.* p. 366. Parisiis, 1728. (2) *Boch. Chan.* p. 812, 813. *Francos. ad Moen.* 1681. (3) *Id. ibid.* p. 561. (4) *Strab. Lib. xi.* p. 523.
(5) *Steph. Byzant.* in voc. ΓΑΖΑΚΑ.

feu tiarâ conspicui nobilitatum, supellex mea antiquaria suppeditat. Hoc ante caput sceptrum ostentat, pone autem figuram, quæ Græcum elementum *Gamma* referre videtur. Cætera nummus noster Bouterouiano non dissimilis jure haud iniquo censendus. Quare altero aliquantulum illustrato, nonnihil etiam lucis alteri forsan aspergatur.

II.

Tiaram quod attinet, nostro in numismate depictum, eum è rectis unum esse, quos, Xenophonte (2) teste, Cyrus accepit à Medis, et ad successores transmisit, summæ majestatis insignia, ita ut nemini liceret gerere, nisi regibus, ex auctoribus hîc laudatis abundè fit perspicuum. “ Solos (ait (3) V. C. Theophilus Sigefridus Bayerus) “ reges gessisse ad-
“ firmant nobis Aristophanes et ejus Scholiasta de
“ Persis, Josephus autem et Dio Prusæus de Parthis.” Pergit Cel. Bayerus, “ Quos igitur (tiaras) dicemus
“ rectos nisi eos, qui non lunatum in orbem circum
“ tempora flecterentur, sed rigidiores essent apices et
“ altiores?—Neque ausi sunt (si eundem audiamus Bayerum) “ Edesseni reges iis uti, potentia et ma-
“ jestate inferiores Parthis.” Quandoquidem igitur hic nummus ante eversum ab Artaxerxe Parthorum imperium videtur procusus, ac proinde ad Persas, saltem posteriores, vix referendus; eum à Parthiæ quodam rege fuisse percussum, etsi tiaræ figura parti adversæ insculpta complurium Parthicorum formæ non ad amissim congruat, omninò certè par est arbitrari.

(2) Xen. *Cyrop.* L. viii. c. 3.

(3) Theoph. Sig. Bayer. *Hist. Osroen. & Edessen. &c.* p. 135, 136. Petropoli, 1734.

III.

His ritè perpenſis, tiaram nobis Parthorum regem indigitare, cordatum quemque vix inficias exiſtimo iturum. Quo conceſſo, Vologeſem III. numiſma noſtrum cudiffe, indicio erunt, forſan certiſſimo, in priori facie ante caput ſceptrum et pone litera *Gamma*, ſi modò figura ſuperiùs memorata pro iſto elemento commodè fatiſ accipi poſſit. Etenim huiuſce principis nummum, à (4) Cl. Vaillantio publici juris factum, ſive typum, ſive modulum reſpiciamus, noſtrum plurimùm referre, vel cæcutienti fat clarè innotefcat. Nil interim mirandum, quòd caput regis imberbis, ſeu potiùs mediocriter barbati, antica primùm oſtentaverit; cum (5) Mannus, regulus finitimus Parthis, in nummulo ſine barbâ depingatur, et nonnulli Parthorum (6) reges (quorum numero ipſe forſan Vologeſes III. accenſeatur) mediocriter tantùm barbati in numiſmatis interdum conſpiciantur.

IV.

Quod ad binos racemos ſpectat, quibus pars nummi noſtri poſterior cernitur ſignata, id quoque quicquid ſit ſymboli Parthorum regem ante oculos ponere fortasſe reperiatur. Numiſma enim argenteum majori formâ penes ſpectatiſſimum virum, Henricum March, armigerum, à V. C. Arthuro Pullinger ex Oriente

(4) J. F. Vaill. *Arſacid. Imper.* p. 364—369. Pariſiis, 1728.

(5) Nicol. Fran. Haym Roman. *Del Teſor. Britan.* Vol. Second. p. 57, 58. In Londra, 1720. *Numiſm. Antiqu. Colleçt.* à Thom. Pembr. et Mont. Gom. Com. P. 2. T. 72. Wiſe *Epist. ad V. Cl. Joannem Maſſon de num. Abgari Regis.* Oxon. 1750.

(6) J. F. Vaill. *Arſacid. Imper.* p. 31, 58, 105, 321, 364, 366, &c.

secum in Britanniam reportatum, in anticâ typum ostentat, Parthico (quo complures nummi (7) insigniuntur) non dissimilem : principem nempe fellæ infidentem, manibusque sagittam gestantem, ante cujus pedes visitur arcus; in posticâ verò senem, sinistrâ tenentem racemum. Si igitur numisma nostrum ad Vologesem III. pertinuisse autumemus, à vero forsan non multùm abludemus. Interim si cordatus quispiam aliter senserit, integrum sit ipsi suâ gaudere sententiâ, eandem quoque nobis largiatur veniam.

V.

Si superiùs annotatis suffragentur eruditi, et a Vologese III. nummulus noster habeatur revera percussus, roget forsan aliquis, “ quâ sit ut princeps iste æreos
 “ cuderit nummos, cum argentei ferè soli (si ex iis
 “ qui nobis supersunt hâc de re iudicium faciamus)
 “ sub ejus in imperio antecessoribus prodierint ? ”
 Cui respondeamus licet, “ Monnesis nummus, te-
 “ tradrachmarum magnitudine (8), à Cl. Vaillantio
 “ evulgatus, pariter ac noster, æreus est; quin et
 “ alium Vologesis II. rarissimum ad æreorum mediæ
 “ formæ numerum idem adscripsit (9) Vaillantius.
 “ Quidnè igitur Vologesis III. numismata interdum
 “ ex ære cernantur conflata? Hoc enim factum
 “ fuisse propter res Parthiæ difficillimas, eo imperii
 “ clavum tenente, summamque argenti penuriam,
 “ cum jam laudato (10) Vaillantio arbitramur.”

(7) J. F. Vaill. ubi sup. pass.

(8) J. F. Vaill. *Arfacid. Imper.* p. 341.

(9) Idem ibid. p. 345.

(10) Id. ibid. p. 341.

VI.

Parthorum res sub Vologese III. haud parùm fuisse turbatas, bello iis à Severo tunc temporis illato, (11) ex Herodiano, Spartiano, et Dione abundè constat. Anno enim Arsacidarum æræ vulgaris 456°. vel 457°. seu potius, si meam eatur in sententiam (12), 429°. Severus Parthos inopinatò est aggressus; quin et cæsis primò *quicunque obvii ei sunt facti*, non *quicunque restitissent*, ut Herodiani (13) verba, πάντα τε τ' ἐμπίπλοντα, perperàm Latinè vertit Angelus Politianus, regiam Ctesiphontem trajecto Tigri cepit, atque militibus diripiendam concessit. Illinc ipso rege cum paucis tantùm equitibus elapso, thesauris illius ornatuque omni et supellectile potiti victores regrediuntur. Quanquam autem, prosperè succedentibus rebus, literas ad Senatum Populumque Romanum de se magnificas dederit, ac depictas in tabulis pugnas, victoriasque suas, si Herodiano (14) fides sit habenda, exposuerit Severus, nihil non honorum decernente Senatu, et super his cognomina imponente gentium devictarum; “ ullam (15) tamen, eodem “ anno, Romanos inter et Barbaros revera interfuisse “ pugnam,” ex isto historico haud faciliè colligamus. Neque ab Herodiano hâc in re dissentit Spartianus (16), qui nullius Severum inter et Vologesum prælii,

(11) Herodian. Lib. iii. Æl. Spartian. in *Sever.* Dio, Lib. lxxv. p. 854.

(12) *Philosoph. Transact.* Vol. L. p. 183—187.

(13) Herodian. ubi sup. c. 30. Oxon. 1678.

(14) Idem ibid. c. 31.

(15) Venut. ad V. C. Joannem Nixonum, *A. M. & R. S. S. Epist.* Vid. etiam *Act. Philosoph. Anglican.* Vol. LI. p. 205.

(16) Æl. Spartian. ubi sup.

dicto anno commissi, usquàm meminit. Quod quidem silentium calculo suo egregiè confirmat Dio (17), cujus sic se fermè habent verba: “ Item Seluciam et “ Babylonem *desertas celeriter occupavit*, ac paulo “ post Ctesiphontem; quam urbem diripiendam reliquit militibus, et in eâ cædem maximam fecit, “ cepitque superstitum hominum ad centum millia. “ Vologesum tamen non est infecutus, neque Ctesiphontem retinuit; sed tanquàm ob id solum venisset, ut eam &c.” Undè verisimile admodum videtur, nullam Romanos inter et Barbaros hâc fortuitâ in Parthiam expeditione interfuisse pugnam; cum Seleuciam et Babylonem ab hostibus fuisse *desertas*, et à Severo *celeriter occupatas*, celeberrimus ille scriptor disertè monuit. Celeberrimus, inquam, ille scriptor; cum Dioni Severi comiti et familiari omninò fidendum, præsertim si suo testimonio dicti duo historici, Spartianus et Herodianus, scriptis ejus etiam fidem adstruxerint. Ast quanquam in suâ ad Senatum epistolâ prælia quædam tantummodò commentus sit Severus, *Victoriam* in averâ parte *Gradientem* præ se ferentes (18), denarios tamen, in perpetuam rei memoriam, signari jussit. Undè quodammodò patet, Romanorum imperatores *Victoriam* in nummis *Gradientem* interdum scalpisse, licet nulla “ inter eos gentemque devictam interfuisset (19) pugna.” Quod et alio forsan exemplo, ex historiâ Romano-Parthicâ petito, clariùs elucescet. In belli adversus Parthos ab Antonino Caracallâ gesti memoriam, prin-

(17) Dio, Lib. lxxv. p. 854.

(18) J. F. Vaill. ubi sup. p. 361.

(19) Venut. ubi sup.

ceps iste (20) nummos argenteos percussit, *Victoriæ Parthicæ* typum exhibentes. Incruentam tamen à Parthis reportavit victoriam. Etenim, “ ingenti cæde
 “ Parthorum factâ, prædâque et hominibus magnâ
 “ copiâ captis, discedit, testante Herodiano (21),
 “ *μηνδεὺς αὐτῷ ἀνδεῖσθαι*, *resistente nullo*, vicisque
 “ et urbibus incensis, datâ potestate militibus rapiendi, quod quisque possit, aut vellet.” Cum Herodiano item, hâc in re (22), concinit Dio; ei enim si fidem tribuamus, Antoninus regionem, quæ Mediam attingit, in quam de improvviso irruerat, magnâ ex parte vastavit, multa castella diruit, Arbela subegit, ac sepulchra regum Parthorum, abjectis ossibus, subvertit. “ Verum (23), ait historicus, *quia*
 “ *Parthi cum eo non venerunt ad manus*, nihil de rebus tunc gestis scribi à me eximium potest.”

VII.

Sed et sententiæ nostræ hîc in medium prolatae calculum quoque suum apposuit præstantissimus Spanhemius. Inter alia enim huc spectantia eruditè annotavit Vir (24) Clarissimus, “ VICTORIAM PARTHICAM, cum in M. Antonini ac L. Veri, tum Severi, Caracallæ, ac Macrini nummis legi, immo in Severi nummis etiam VICTORIAM PARTHICAM MAXIMAM: à quibus utique principibus *bella feliciter contra Parthos gesta me-*

(20) J. F. Vaill. ubi sup. p. 375.

(21) Herodian. ubi sup. Lib. iv. c. 20. p. 163. Edit. Oxon. 1678.

(22) Dio, Lib. lxxvii. p. 881.

(23) Idem ibid.

(24) Ez. Spanhem. *De Præstant. & Uf. Numism. Antiquor.* Vol. Alter. p. 635. Amstelædami, 1717.

“ morant

“ morant Romanarum rerum scriptores, ac firmant
 “ insuper, cum iidem nummi, tum additum in iis
 “ PARTHICI, et quidem subinde PARTHICI
 “ MAXIMI, cognomentum.” Notatu hîc non indig-
 num, numismata Romana VICT. PART. vocibus
 dimidiatis in aversâ parte exornata *Victoriæ* itidem
 figuram præ (25) se ferre. Undè omninò
 patet, Romanos in nummis VICTORIAM PARTHICAM,
 auctore Cel. Vir. Ezech. Spanhemio, sculpsisse (26), si *bella feliciter contra Parthos*, vel
 prælio secundo, vel alio quoquo modo, *gererentur*.

VIII.

Hæc de monetam *Victoriâ* signandi more, Romanis diu usitato, dicta sunt. Ad Parthos quod attinet, ex nummis talem apud eos consuetudinem (27) non invaluisse constat. *Victoria* igitur, sive potius *Victoria Gradiens*, Latinum ad modum insculpta, certissimo est indicio, Monnesen numisma hanc ostentans (28), à me fusiùs olim atque uberius explicatum, in rerum contra Romanos feliciter gestarum memoriam, utque iis par pari referret, præsertim ob signatam simili typo pecuniam à M. Aurelio Antonino et L. Vero victoribus (29),

(25) J. F. Vaill. ubi sup. p. 333, 375, 382, &c. Francisc. Mediobarb. Birag. *Imperator. Romanor. Numism. &c.* pass.

(26) Ez. Spanhem. ubi sup.

(27) J. F. Vaill. *Arsacidar. Imper.* Nicol. Fran. Haym Roman. *Del. Tesor. Britan.* Vol. Second. p. 30—37. Mus. Theupol. p. 1221, 1222. *Numism. Antiqu. &c.* Thom. Pembr. & Mont. Gom. Com. &c. P. II. T. 76. Claude Bouterouë, ubi sup. aliique Scriptor. pass.

(28) *Philosopb. Transact.* Vol. L. p. 175—188.

(29) J. F. Vaill. ubi sup. p. 333.

percussisse.

percussisse. At prælium exinde tunc temporis Romanos inter et Parthos fuisse commissum, propter rationes superiùs allatas, haud æquo jure colligamus. Quare leviter saltem lapsus videtur (30) V. C. Edvardus Corfinus, (31) “ ubi dubitat, ad epocham “ statuendam, de *Victoriâ* in posticâ (regis Mon-

(30) V. C. Edv. Corfin. apud Venut. in *Latin. ad Joan. Nixonum Epist.*

(31) Perpenso (quod sequitur) Cl. Nixoni apographo, ab eo mecum quàm humanissimè communicato, de vero hîc sensus acumine cordatus lector liquidius judicare possit. “ Cl. Corfino doctissimas Cl. Swintoni conjecturas de nummo Monnesis Parthorum regis communicavi: ille vero comprobavit, admiratusque est solertiam authoris, ingeniique acumen; tantummodò ubi “ *dubitat*, ad epocham statuendam, de *Victoriâ* in posticâ exculptâ, cum eo anno nulla interfuerit pugna inter Romanos et “ Barbaros: sed cum Monneses regnum usurpaverit, legitimosque “ spoliaverit hæredes, vicinaque devastaverit regna, nil mirum si “ *Victoriam* in nummis insculperit, atque ad Barbaricam, non “ ad Romanam, referri debet.” Hinc abundè constat, Corfinum, non me, ut voluit Cl. Nixonus, “ ad epocham statuendam, de “ *Victoriâ* in posticâ exculptâ dubitâsse.” Nemini enim non liquet, meam de verâ Arfacidarum ærâ, hujusce *Victoriæ* typi ope, feliciùs enodandâ sententiam, quanquam immeritò, in dubium vocâsse Cl. Corfinum; “ cum eo anno nulla, si illum audimus, interfuerit pugna inter Romanos et Barbaros.” En causâ, cur, “ ad epocham statuendam, de *Victoriâ* in posticâ exculptâ “ dubitaverit” Corfinus, ab ipso Corfino, vel, si malimus, Venuto, in medium prolata. Quinimò *Victoriæ* figurâ, in hoc Monnesis numismate scalptâ, æram Parthorum Corfinianam (vel invito Corfino) sine ullâ dubitatione firmandam duxi, ut ex nostris *Ætis Philosophicis* omninò sit perspicuum. Voces igitur *tantummodò ubi dubitat* minùs rectè but excepted a little to that part, where he [Mr. Swinton] expresses a doubt, siquid judico, Anglicè vertit Cl. Nixonus. Neque meo solo, hâc in re, nixus sum judicio; cum præcellentes, quotquot consului, eruditione amici, inter quos eminet reverendus ac doctissimus vir, Josephus Sanford, S. T. B. Collegii Balliolensis Oxon. Socius, mihi certatim adstipularentur. *Philosoph. Transact.* Vol. L. p. 185.

nefis nummi) exculptâ, cum eo anno nulla
 “ interfuerit pugna inter Romanos et Barbaros ;”
 tametsi meam hujusce numismatis explicationem,
 quod summo mihi honori duco, teste Cl. Venuto (32), comprobavit.

IX.

Sed et minùs sibi constare, si non apertè secum
 pugnare, quoque videtur vel Cel. Corfinus, vel doctissimus Venutus ; cujus huc spectantia, in pereruditâ suâ ad V. C. Joannem (33) Nixonum epistolâ, ita se habent verba. “ Sed cum Monneses regnum
 “ usurpaverit, legitimosque spoliaverit hæredes, vicinaque devastaverit regna, nil mirum si *Victo-*
 “ *riam* in nummis insculpsit, atque ad Barba-
 “ ricam, non ad Romanam, referri debet.” Hinc enim abundè constat, docente nòs vel Corfino, vel Venuto, Monnesen monetam suam *Victoriâ* signâsse, proptereâ quod “ regnum usurpaverat, legitimosque spoliaverat hæredes, vicinaque devastaverat regna,” quanquam de prælio inter eum et Barbaros commissò omninò fileat historia. Ast hoc minimè patitur instituti Corfiniani ratio, quandoquidem antea “ dubitaverat de *Victoriâ* in posticâ
 “ exculptâ” Vir doctissimus, “ cum eo anno nulla
 “ interfuerit pugna inter Romanos et Barbaros,” si ex historicorum silentio id evinci possit. Pari igitur jure *Victoriam* Barbaricam, si hujusmodi valeat ratio, ac Romanam repudiemus.

(32) Venut. ubi sup.

(33) Idem ibid.

X.

Sed ut verum apertè atque ingenuè fateamur, id genus argumenta, ex historiæ silentio ducta, facile et nullo negotio, advocatâ præsertim nummorum fide, haud rarò refelluntur. Victorias Romanorum copiis partas præstantiora interdum memorant numismata, ab eorum scriptoribus silentio penitus prætermittas; cujus, ut alia nunc non in medium proferam, nobis exemplo fit VICTORIA GOTTHI. P. (34) à Taciti imperatoris nummo ante oculos posita, et cæteroquin (ut opinor) minimùm nota. Parthos igitur Romanorum aciem non profligasse, eodem quo Mesopotamiæ urbes ceperunt anno, utcunque de pugnâ taceat historia, pro certo dicere non ausim. Immò Parthos cum Romanis tum dimicasse à vero non usquè adeò forsan alienum reperietur, si Vaillantium, cujus de Monnese sensa pressè satis sequitur (35) Cl. Corfinus, perpendamus (36) innuisse, “ hunc principem ΣΩΤΗΡΟΣ epitheton sibi adrogavisse, ob “ egregium quoddam adversus Romanos facinus bel- “ licum; quo populorum omnium sibi benevolen- “ tiam atque amorem conciliavit, quoque in turbu- “ lento illo rerum statu ad Parthicum solium viam “ sibi patefecit.” Utut tamen est, nil mirum si *Victoriam*, more Romano *Gradientem*, Monnesis nummus jam ostendet; cum bella (37) contra Romanos feliciter gesserit, et totam fere Mesopotamiæ provinciam, secundum (38) Dionem, perdomuerit.

(34) Francisc. Mediobarb. Birag. ubi sup. p. 410. Mediol. 1683.

(35) Edv. Corfin. apud Venut. ubi sup.

(36) J. F. Vaill. ubi sup. p. 340.

(37) Idem ibid.

(38) Dio, Lib. lxxv. p. 853. *Philos. Transf.* Vol. L. p. 184, 185.

LXVII. *An Account of a stony Concretion
taken from the Colon of an Horse; by Mr.
H. Baker, F. R. S.*

Read May 8,
1760.

I Have now the honour to lay before this Royal Society one half of a stony concretion, formed in the colon of an horse, which was lately sent me from Norwich, by my very curious friend Mr. William Arderon, F. R. S. This horse, he informs me, was twenty-five years old, had been often ill, and under the farrier's care; which made him desire to have it opened, when dead.

The whole stone, when first taken out, weighed ten pounds and half; and he caused it to be sawed in two, that a better judgment might be made of the manner of its formation. Several smaller concretions were also found in the colon, some whereof are shewn herewith; but they are much less solid, and more irregular, than the large one.

This ball is full seven inches in diameter, and consists of many laminæ, or coats, which form a number of concentric circles, around a nucleus in the center, which seems to be a small shiver of black flint. Fifteen or sixteen of these coats are easily distinguishable, and some have been broken off: they vary something in colour, and are, in general, so stony, that they would probably take a pretty good polish. The coats differ in thickness, according, perhaps, to the time they were in forming: for it should seem, if conjecture may be allowed, that each coat was formed in a longer or shorter time, according to
its

its thickness; and that, between the finishing of one coat and the beginning of the next, there was some interval of time, and some suspension of that attractive power whereby, or of that component matter whereof, the several coats are respectively formed.

Strand, May 8, 1760.

H. Baker.

LXVIII. *An Explanation of the Modes or Tones in the antient Græcian Music; by Sir Francis Haskins Eyles Stiles, Bart. F. R. S.*

Read December 1759, and January, &c. 1760.

Notwithstanding the valuable editions of the antient musical and harmonic writers, given by Meibomius and Wallis, and the great pains those two learned editors have bestowed on the correcting the tables, and throwing light on the difficult passages of those works, there are some of the doctrines delivered in them, that seem still to require a more perfect explanation. Those, that respect the tones or modes, in particular, seem to have been ill understood: and as it was on these, that the effects of the antient music were supposed principally to depend, I have thought, that the subject might well merit a re-examination.

Concerning these modes, we find two distinct, and seemingly contradictory doctrines delivered by the antients; and this it is, which has perplexed the subject;

ject; for some, not aware of the distinction, have charged the antients with contradictions; and others, who perceived the two doctrines, not being able to reconcile them, have either adopted one, and rejected the other, or given up the subject as hopeless: but, as they were both admitted by the antients, they must both have been true, in some sense. What, therefore, I have principally in view in these sheets, is to shew, that the difference between the doctrines arose only from the different way of considering one and the same object; and that therefore there was such an agreement betwixt them, as that, under certain restrictions, they may be embraced under one common interpretation.

For distinction sake, I shall call one of these doctrines the harmonic, and the other the musical doctrine; the reason of which will sufficiently appear, when I come to treat of the distinction between the science of harmonic and that of melopœia or musical composition.

According to the harmonic doctrine, the number of the modes had been augmented to fifteen; but as Ptolemy, who appears to have favoured the musical, reduces them to seven, and as it is on the principles of that writer, that I propose to shew an agreement between the doctrines, it will be necessary for me, in treating of the harmonic modes, to distinguish the seven he admits from the eight he has excluded.

This being premised, I shall be understood in making the distribution of what I have to offer upon this subject, which I propose to treat under the following six heads.

First,

First, To explain the harmonic doctrine, as far as concerns the seven modes admitted by Ptolemy.

Secondly, To explain the musical doctrine of the same modes, and shew its agreement with the harmonic.

Thirdly, To explain the eight harmonic modes rejected by Ptolemy, and give at large his reasons for excluding them.

Fourthly, To point out something of the origin of the two doctrines.

Fifthly, To shew how far the preceding explanations may be supported by arguments, or warranted by the testimony of antient writers.

Lastly, To consider how this subject has been understood by Meibomius, Wallis, and some few others that have wrote since, and in what respect their explanations differ from my own.

I now proceed to the first head; *viz.*

First, To explain the harmonic doctrine, as far as concerns the seven modes admitted by Ptolemy.

The harmonic doctrine of the tones we find expressly delivered in the harmonic treatises; which, probably, is the reason, why, among the moderns, the later writers at least have, for the most part, determined themselves in its favour: how justly it is intitled to the preference, I shall have sufficient occasion to examine. The treatises on harmonic, that are come down to us, are mostly Aristoxenian, which school seems to have treated the science, if not better, at least

least more methodically than any other, of which we can now form a judgment; and hence we find their divisions often adopted by ancient writers, who might not, perhaps, be ready to admit all their principles. In considering the physical properties of sound, and the ratios of intervals, the Aristoxenians appear to have been less exact than the Pythagoreans, the doctrines of the former being more adapted to the gross and familiar notions of the practical musician, than to the accurate speculations of the philosopher. But, however exceptionable their treatises may have been in this respect, they are the more valuable to us, on this very account, as they give more light into the ancient practice of music; which is what is chiefly desired, the philosophic principles of the science being better understood. By this school harmonic was divided into these seven parts; 1. of sounds, 2. of intervals, 3. of genera, 4. of systems, 5. of tones, 6. of mutations, 7. of melopœia. The propriety of their adding this last division I shall have occasion to consider. Of these divisions, it was the fifth, which contained the doctrine in question; but, to complete it, the sixth must also be taken in; for, amongst other mutations, that of the tones was there treated of, and was indeed the most considerable object of that division. This doctrine taught, that the difference between one tone and another lay in the tension or pitch of the system. The system (by which I mean the greater perfect one, exclusive of the less, of which I shall have little occasion to speak) consisted of fifteen sounds, extending to a disdiapason, or double octave. How these sounds were denominated, and at what intervals they succeeded each other, in
the

the diatonic genus, to which I shall confine this explanation, is so well known, that I need say no more concerning the structure of the system, than that it answered to that of our natural scale, beginning with *Are*, and ending with *Alamire*. This system was held immutable, as to the relation of its parts one to another; that is, the order of sounds and intervals, by which it proceeded, was in the same genus to be always the same; but the tension or pitch of the whole was variable, a different one being assigned for each mode. The explanation, therefore, usually given of the system, by comparing it, as I have done, to a double octave, from *A* in our natural scale, is not to be understood as fixing its pitch, but as shewing its succession only; which might as well be done by a double octave from *B*, taking *C#* and *F#* into the scale, or from any other note, taking in the flats and sharps necessary to make the tones and semitones follow in the same order.

The relation of the parts to each other in the system being immutable, the fixing the pitch of any one sound in it, for any mode, was sufficient to determine that of all the rest. For this purpose, the sound *mese* was commonly used; which, by its situation, was well adapted to it, being the middle sound of the system. If, therefore, we settle the position of the *mese* for each mode, we shall do all, that is requisite for the clearing up and establishing the harmonic doctrine, which considered the modes as differing only in the pitch of the system.

The modes admitted by the *Aristoxenians* were thirteen in number; to which two more were added by later harmonicians; and to the *mese* of each of

these fifteen a distinct pitch was assigned ; but, as Ptolemy has rejected eight of them, I shall, as I have proposed first, separately consider the positions of the mese for the seven modes he admits.

Concerning the relative pitch of the respective meses for these seven modes, I find no disagreement amongst the harmonic writers. There are not wanting, indeed, who charge the antients with giving, in respect thereto, contradictory accounts. Amongst others, the learned Dr. Gregory asserts, that Aristides Quintilianus inverts the order of the modes (1) : but what led the doctor into this mistake, was his not distinguishing the double doctrine. Aristides, in the passage cited, is not speaking of the pitch of the system for the seven modes in question, but of the seven species of diapason, as they lay in the system ; which was, indeed, in the inverted order of the meses of the seven modes, as will appear, when we come to consider the other doctrine. This, then, is no contradiction in the Greek writer, nor, if it were, would it be chargeable singly on him ; since, if the doctor had but cast his eye on his own Euclid (2), he would have met with the very same doctrine he found in Aristides. But his remark is, indeed, entirely without foundation ; and I make no scruple to assert, that the antients agree in their accounts of the relative pitch of the meses, for these seven modes ;

(1) Atque Aristides Quintilianus (pag. 18. editionis Meibomianæ) tonum Hypodorium acutiorē facit quā Dorium, et Hypophrygium quā Phrygium, et Hypolydium quā Lydium. Vide Præfat. ad Opera Euclid.

(2) Pag. 540. vers. 6. et seq. seu pag. 15. vers. 15. edit. Meibom.

for though many of the harmonic writers treat of the thirteen or fifteen modes all together, without distinguishing the seven in question, yet they have given them in such order, and at such distances, that the seven stand at the same intervals from each other, as are assigned for them by those, who have treated of them separately; so that all the accounts agree in this particular. The direction we may conveniently take from Bacchius (3), where it is clearly and concisely delivered; and where it appears, that the Mixolydian was the most acute; the Lydian, graver by a semitone; the Phrygian, graver than the Lydian by a tone; the Dorian, graver than the Phrygian by a tone; the Hypolydian, graver than the Dorian by a semitone; the Hypophrygian, graver than the Hypolydian by a tone; and the Hypodorian, graver than the Hypophrygian by a tone. Now, as the Guidonian scale, in use amongst the moderns, when stripped of Guido's additions, answers to the system of the antients, in its natural situation, which was in the Dorian mode, and our Alamire consequently answers to the pitch of the Dorian mese, we have a plain direction for finding the absolute pitch of the meses, for all the seven in our modern notes, and they will be found to stand thus:

Mixolydian mese in	<i>d</i>	Hypolydian in	- - <i>g</i> [#]
Lydian in	- - - <i>c</i> [#]	Hypophrygian in	- - <i>f</i> [#]
Phrygian in	- - - <i>b</i>	Hypodorian in	- - - <i>e</i>
Dorian in	- - - <i>a</i>		

But to understand this doctrine, as it is delivered by the antients, it will be necessary also to examine, how

(3) Pag. 12. edit. Meibom.

the mēses of the seven modes were stationed upon the lyre; and, in order to this, we must consider the structure of that instrument. The lyre, after its last enlargement, consisted of fifteen strings, which took in the compass of a disdiapason, or double octave. These strings were called by the same names as the fifteen sounds of the system, and when tuned for the Dorian mode, corresponded exactly with them. Indeed there can be no doubt, but that the theory of the system had been originally drawn from the practice of the lyre in this mode, which was the favourite one of the Greeks, as the lyre was also their favourite instrument. In this mode, then, the mēse of the system was placed in the mēse of the lyre; but in every one of the rest, it was applied to a different string, and every sound of the system transposed accordingly. Hence arose the distinction between a sound in power and a sound in position; for when the system was transposed from the Dorian to any other mode, (suppose, for instance, to the Phrygian) the mēse of the lyre, though still mēse in position, acquired, in this case, the power of the lichanos mēson; and the paramēse of the lyre, though still paramēse in position, acquired the power of the mēse. In these transpositions, one or more of the strings always required new tunings, to preserve the relations of the system; but, notwithstanding this alteration of their pitch, they retained their old names, when spoken of, in respect to their positions only; for the name implied not any particular pitch of the string, but only its place upon the lyre, in the numerical order, reckoning the proslambanomenos for the first.

I thought:

I thought this remark the more necessary, as I suspect it was the not attending to this circumstance, that led Dr. Wallis to assign *c* and *g* natural, for the places of the mese in the Lydian and Hypolydian modes, instead of *c*[#] and *g*[#], where I have placed them (4). But, to return,

We are now to give the places of the mese for these seven modes upon the lyre, which are thus settled by Ptolemy (5).

		String
Mixolydian	Mese in {	Paranetediezeugmenon 11
Lydian		Tritediezeugmenon - 10
Phrygian		Paramese - - - 9
Dorian		Mese - - - - 8
Hypolydian		Lichanos meson - - 7
Hypophrygian		Parhypate meson - - 6
Hypodorian		Hypate meson - - - 5

The positions assigned for the meses of these seven modes, by the harmonic doctrine, being thus settled, both in our modern scale, and upon the lyre, I come now,

Secondly, To explain the musical doctrine of the same modes, and shew its agreement with the harmonic.

The musical doctrine taught, that the difference between one mode and another consisted in the manner of dividing an octave, or, as the antients express it, in the different species of diapason. The elementary principles of these species we find delivered

(4) See his edition of Ptolemy, p. 137.

(5) Ibidem.

in the harmonic writers; but as in the Aristoxenian treatises, they are not found under the fifth division, of tones; nor under the sixth, of mutations, where it was natural to expect them; but under the fourth, of systems; and it not being there expressly affirmed, that the species had a relation to the tones, though, from their denominations, and other circumstances, it might well be inferred, this has created a difficulty in admitting their connection with the subject. The obviating of this objection I shall reserve for my fifth head; where I shall take occasion, particularly, to justify this doctrine, and the use I shall have made of it; and I shall therefore proceed to explain it, as I find it in the harmonic writers.

To understand how this doctrine of the species of diapason came to be treated under the head of systems, it will be necessary to consider the definition given of the term system by the Aristoxenians, and their subdivision of this branch of harmonic.

With this school, whatever consisted of more than one interval, was a system. So comprehensive a definition could not but make this branch a very large one; and so we accordingly find it. It was subdivided in the manner following: systems were there considered as differing in respect, first, to magnitude; secondly, to genus; thirdly, to the being consonant or dissonant; fourthly, to the being rational or irrational; fifthly, to the being sequent or transgressive; sixthly, to the being conjunct or disjunct; seventhly, to the being mutable or immutable. Now, of these seven differences, it was under the third that the doctrine of the species of diapason came to be treated, the consonant systems being there enumerated, and explained.

explained. By a consonant system was understood a system, whose extreme or comprehending sounds were consonant: of these, there were six within the compass of the immutable system, *viz.* the diatessaron, diapente, diapasos, diapasos and diatessaron, diapasos and diapente, and disdiapasos. These consonant systems were considered as admitting each of a variety in the order of the intervals, of which it was composed. A system of a determined magnitude, composed of the same intervals, and of the same number of them, might, if the component intervals were not equal, differ in respect to their order or succession. These variations of each system they called *εἶδος*, its species, or *σχημα*, its figure, for these terms were synonymous (6). The species of diatessaron were three; those of the diapente four, and those of the diapasos seven, being the sum of the species of the other two consonances, of which the diapasos itself was composed: and here the doctrine of the species stopped; for, in the three larger consonant systems, they seem not to have considered the species of the whole magnitude, but only those of the three smaller systems, of which they were composed, and which are called, by Ptolemy, the first consonances (7). As the species of diatessaron and diapente do not immediately concern this doctrine, the explaining them will be unnecessary; and I shall therefore proceed to those of the diapasos.

(6) Μετὰ δὲ ταῦτα λεκτέον, τίς ἐστὶ, καὶ ποία τις, ἢ κατ' εἶδος διαφορὰ; διαφέρει δ' ἡμῖν ἕδεν, εἶδος λέγειν ἢ σχῆμα· φέρομεν γὰρ ἀμφοτέρω τὰ ὀνόματα ταῦτα ἐπὶ τὸ αὐτό. Aristox. p. 74. v. 9.

(7) Περιδιδείκναι τὰς κατὰ τὸ καλέμενον εἶδος τῶν πρώτων συμμετρίων διαφορὰς. Ptol. lib. ii. c. 3.

Concerning these, I find no disagreement amongst the writers of antiquity: they all agree, that they were seven in number, and had the denominations of Mixolydian, Lydian, Phrygian, Dorian, Hypolydian, Hypophrygian, and Hypodorian; circumstances, which leave no doubt of their connection with the modes. The succession of intervals, or manner of dividing the diapason for each species, has been variously explained by the antient writers; but the result of these explanations is the same, excepting the generic differences. It will be sufficient here to give what respects the diatonic genus only, as the question concerning the tones does not turn upon the distinction of the genera.

In the diatonic genus, the diapason consisting of five tonic and two semitonic intervals, the Aristoxenians fixed the succession for each species, by the position of the two semitones; shewing, at the same time, between which of the sounds of the immutable system the species in question was comprehended: for this system consisting of fifteen sounds, contained eight octaves, two of which, *viz.* that from *proslambanomenos* to *mese*, and that from *mese* again to *netehyperboleon*, were divided after the same manner, and therefore constituted the same species: but the six others were all differently divided; so that the system contained within itself the seven species of diapason, and thence obtained the denomination of perfect; the lesser perfect system, which reached only to a diapason and diatessaron, being improperly so called for want of this qualification, as Ptolemey
has

has shewn (8). According to this method of explaining the species of diapason, they stand as follows :

Species,	
1. Mixolydian	From hypate hypaton to paramese. Semitones, first interval in the grave, fourth in the acute.
2. Lydian	From parhypate hypaton to trite diezeugmenon. Semitones, third interval in the grave, first in the acute.
3. Phrygian	From lichanos meson to paranete diezeugmenon. Semitones, second interval in the grave, second in the acute.
4. Dorian	From hypate meson to nete diezeugmenon. Semitones, first interval in the grave, third in the acute.
5. Hypolydian	From parhypate meson to trite hyperbolæon. Semitones, fourth interval in the grave, first in the acute.
6. Hypophrygian	From lichanos meson to paranete hyperbolæon. Semitones, third interval in the grave, second in the acute.

(8) "Ὁθεν τὸ συντιθέμενον ἐκ τῶ διαπασῶν καὶ διὰ τεσσάρων σύστημα τέλειον ὡς καλῶς ἔχει καλεῖν· τὰ μὲν γὰρ ἐπὶ αἰδῆ τῶ διαπασῶν ἔστι ποτε περιέξει. Ptol. Harm. p. 106. v. 3.

Species.

7. Hypodorian

{ From mese to nete hyperbolæon,
or prosl. to mese.
Semitones, second interval in the
grave, third in the acute.

It must be here observed, that these species, as they stand in the system, are, with respect to acuteness and gravity, in the inverted order of the seven modes, as settled by the harmonic doctrine; which will account for the mistake, which Dr. Gregory (as was shewn above) and others have fallen into, in imagining the antients did not agree in their accounts of the modes; since it is plain these writers did not observe, that, in the one case, the modes were spoken of with respect to the position of the system itself, and in the other, with respect to the position of the seven species within the system.

These species may also be more readily shewn, by examples out of the Guidonian scale, where, keeping the natural notes, they will stand thus:

Mixolydian.	}	from	{	B to <i>b</i> .
Lydian				C to <i>c</i> .
Phrygian				D to <i>d</i> .
Dorian				E to <i>e</i> .
Hypolydian				F to <i>f</i> .
Hypophrygian				G to <i>g</i> .
Hypodorian				<i>e</i> to <i>a a</i> , or A to <i>a</i> .

But as well this, as the former way of shewing them, we are to understand only as exemplifications of the succession of the intervals, and not as an assignment of the pitch for each, which was to depend on that of the system, out of which they are exemplified; and

and it may not be amiss therefore, in order to prevent any limited conception of these species, to state generally the order of the intervals, of which each is composed; which will be as follows, proceeding in each from grave to acute.

Mixolydian.	Semitone, tone, tone, semitone, tone, tone, tone.
Lydian.	Tone, tone, semitone, tone, tone, tone, semitone.
Phrygian.	Tone, semitone, tone, tone, tone, semitone, tone.
Dorian.	Semitone, tone, tone, tone, semitone, tone, tone.
Hypolydian.	Tone, tone, tone, semitone, tone, tone, semitone.
Hypophrygian.	Tone, tone, semitone, tone, tone, semitone, tone.
Hypodorian.	Tone, semitone, tone, tone, semitone, tone, tone.

Besides the manner above-mentioned of explaining the species of diapason, the antients have given us another, the result of which is the same; and that is, by the position of the diazeuctic tone, or interval from mese to paramese. In the Mixolydian species, the diazeuctic tone was the first interval, reckoning from acute to grave; in the Lydian, it was the second; in the Phrygian, the third; in the Dorian, the fourth; in the Hypolydian, the fifth; in the Hypophrygian, the sixth; and in the Hypodorian, the last.

Now, either of these methods fixes the succession of intervals peculiar to each species; but as the examples are taken from a system, whose pitch was variable, we are still to seek, at what absolute pitch the several species were taken in the modes, to which we suppose them to have been respectively subservient; and it is, perhaps, the seeming difficulty of settling this, that has induced so many to reject this doctrine entirely, and fall in with the harmonic writers, who considered the modes as differing only

in the pitch of the system: but, by taking in the assistance of the harmonic doctrine, we shall not only gain the true pitch for each species in the musical, but be led to see the agreement between the two doctrines, which, I have already said, was my principal view in these sheets, and which, having gone through the explanation of each doctrine separately, as far as concerns the seven modes, we are now ripe for considering; for the eight modes, whose explanation I have deferred, have no concern in this agreement, being rejected by Ptolemey, as will appear, for this very reason, in substance, that they would stand the trial of the harmonic doctrine only, whereas the rest had the support of both. Let us then consider how the two doctrines, as I have explained them, may be made to agree.

By the harmonic doctrine, we are told the pitch of the system for each mode; and by the musical, in what part of the system to take the species of diapason: now, by combining these two directions, we gain the following plain canon, for finding any mode required.

CANON.

First pitch the system for the mode, as directed by the harmonic doctrine; then select from it the diapason, directed by the musical; and we have the characteristic species of the mode in its true pitch.

To make this more plainly appear, and also to avoid the length of particular explanations, I have annexed a diagram of the seven species, which will shew at what pitch of the Guidonian scale each sound
of

DIAGRAM I. Of the Species of Diapason in the Seven Modes admitted by PROLEMEY.

Mixolydian.	Lydian.	Phrygian.	Dorian.	Hypolydian.	Hypophrygian.	Hypodorian.
<i>e</i> Paramese. Diaz. tone. <i>d</i> Mese. <i>c</i> Lich. mese. <i>b</i>^b Parhyp. mese. <i>a</i> Hyp. mese. <i>g</i> Lich. hyp. <i>f</i> Parhyp. hyp. <i>e</i> Hyp. hyp.	<i>e</i> Trita diez. <i>d</i>[#] Paramese. Diaz. tone. <i>c</i>[#] Mese. <i>b</i> Lich. mese. <i>a</i> Parhyp. mese. <i>g</i>[#] Hyp. mese. <i>f</i>[#] Lich. hyp. <i>e</i> Parhyp. hyp.	<i>e</i> Paran. diez. <i>d</i> Trita diez. <i>c</i>[#] Paramese. Diaz. tone. <i>b</i> Mese. <i>a</i> Lich. mese. <i>g</i> Parhyp. mese. <i>f</i>[#] Hyp. mese. <i>e</i> Lich. hyp.	<i>e</i> Nete diez. <i>d</i> Paran. diez. <i>c</i> Trita diez. <i>b</i> Paramese. Diaz. tone. <i>a</i> Mese. <i>g</i> Lich. mese. <i>f</i> Parhyp. mese. <i>e</i> Hyp. mese.	<i>e</i> Trita hyperb. <i>d</i>[#] Nete diez. <i>c</i>[#] Paran. diez. <i>b</i> Trita diez. <i>a</i>[#] Paramese. Diaz. tone. <i>g</i>[#] Mese. <i>f</i>[#] Lich. mese. <i>e</i> Parhyp. mese.	<i>e</i> Paran. hyperb. <i>d</i> Trita hyperb. <i>c</i>[#] Nete diez. <i>b</i> Paran. diez. <i>a</i> Trita diez. <i>g</i>[#] Paramese. Diaz. tone. <i>f</i>[#] Mese. <i>e</i> Lich. mese.	<i>e</i> Nete hyperb. <i>d</i> Paran. hyperb. <i>c</i> Trita hyperb. <i>b</i> Nete diez. <i>a</i> Paran. diez. <i>g</i> Trita diez. <i>f</i>[#] Paramese. Diaz. tone. <i>e</i> Mese.



of the diapason is brought out by the canon for each of the seven modes. As in the construction of this diagram, the directions of the canon have been strictly pursued, so it will appear, that the result of it is, in all respects, conformable to the principles of both doctrines. Thus in the Dorian, for instance, it will be seen, that the mese is placed in Alamire, and that the rest of the sounds exhibited in that diapason, are placed at the proper distances, for preserving the order of the system, as required by the harmonic doctrine. It will be also seen, that the diapason selected lies between hypate meson and nete diezeugmenon; that the semitones are the first interval in the grave, and third in the acute; and that the diazeuctic tone is in the fourth interval, reckoning from the acute; all which circumstances were also required by the musical doctrine for this mode; and, in the rest of the modes, all the circumstances required by each doctrine will, in like manner, be found to obtain: so that no objection can well be raised to the principles, on which the diagram has been framed, by the favourers of either doctrine separately; and the very coincidence of the two doctrines therein, might furnish a probable argument in justification of the manner, in which I have combined them in the canon. But as I propose to consider this under the fifth head, where the proofs will be collected, I shall leave this argument for the present, and proceed to explain,

Thirdly, The eight harmonic modes rejected by Ptolemey, and give at large his reasons for excluding them.

Six of these modes, with the seven Ptolemey allowed, made up the thirteen allowed by the Aristoxenians: the other two seem to have been added afterwards, more with a view to regularity in the names and positions of the modes, than to any particular use, as will appear in the course of the explanation. For the settling these eight modes, I shall have no occasion to go farther than the fixing the position of the mese for each, according to the harmonic doctrine: the result of applying the musical doctrine to them will sufficiently appear, when I come to explain the reasons, which Ptolemey has assigned for rejecting them.

The old Dorian, Phrygian, and Lydian modes, having, as we have seen, their meses respectively in *a*, *b*, *c*#, at the distance of a tone from each other, these tonic spaces were afterwards divided, to make room for the Ionian mese in *b* flat, and the Æolian in *c* natural. To these five modes, whose meses succeeded each other at the interval of a semitone, and which came, in time, to be called the middle modes, five others were made to correspond, that lay respectively graver by a fourth, and took their denominations from them, but compounded with the preposition *hypo*, to distinguish them. Of these, we have already shewn the Hypodorian mese to have been in *e*, the Hypophrygian in *f*#, and the Hypolydian in *g*#. Now, the two tonic spaces between *e*, *f*#, and *g*#, being likewise divisible, the Hypoionian mese was inserted in *f* natural, and the Hypoæolian in *g* natural, at a fourth respectively from the Ionian and Æolian. To these ten modes, it was conceived that five more might properly be ranged
towards

towards the acute, at the distances of a fourth respectively from the five first mentioned, and taking their denominations from them also, but compounded with the preposition *hyper*, which would complete them to fifteen, divided into five grave, five middle, and five acute. But the Aristoxenians limiting the positions of the mese to the compass of an octave, had established only three out of these last five, *viz.* the old Mixolydian, whose mese we have shewn to be in *d*, at a fourth from the Dorian, and which was therefore afterwards called Hyperdorian; the Hyperionian, whose mese was in *e* flat, at a fourth from the Ionian; and the Hypermixolydian (for so it had been originally named, the preposition being taken in a different sense); the mese of which was in *e*, at a fourth from the Phrygian, and which was therefore afterwards called the Hyperphrygian. To make these acute modes therefore five in number, as well as the grave and middle ones, two new modes were added, *viz.* the Hyperæolian, whose mese was in *f*, and the Hyperlydian, whose mese was in *f*[#], at a fourth respectively from the Æolian and Lydian.

The modes being thus augmented to fifteen, upon the single principle of the harmonic doctrine, their meses will be found to stand all together in the following order.

Acute	{	Hyperlydian	-	-	-	-	-	-	in <i>f</i> [#]
		Hyperæolian	-	-	-	-	-	-	<i>f</i>
		Hyperphrygian, or Hypermixolydian	-	-	-	-	-	-	<i>e</i>
		Hyperionian	-	-	-	-	-	-	<i>e</i> b
		Hyperdorian, or Mixolydian	-	-	-	-	-	-	<i>d</i>

Middle

Middle	{	Lydian	-	-	-	-	-	-	-	in $c^\#$
		Æolian	-	-	-	-	-	-	-	c
		Phrygian	-	-	-	-	-	-	-	b
		Ionian	-	-	-	-	-	-	-	b^b
		Dorian	-	-	-	-	-	-	-	a
Grave	{	Hypolydian	-	-	-	-	-	-	-	in $g^\#$
		Hypoæolian	-	-	-	-	-	-	-	g
		Hypophrygian	-	-	-	-	-	-	-	$f^\#$
		Hypoionian	-	-	-	-	-	-	-	f
		Hypodorian	-	-	-	-	-	-	-	e

Having now shewn, how these fifteen modes were situated according to the harmonic doctrine, I come to the objections raised by Ptolemey against the eight last explained. For the right understanding of which, it will be necessary to give an account of all that he has delivered on the subject of the modes, a task of some difficulty; for though his reasoning appears to me most clear and methodical, it is rather too prolix to be given exactly in his own words, as this, with the necessary explanations of those passages, of which the sense is less obvious, would carry me to too great a length. I must therefore endeavour to abridge his doctrines, though at the hazard of leaving out some of the substantial parts, and thereby doing injustice to his argument.

It will be expedient to premise something concerning his apparent intention, and method of reasoning in this part of his work, which will serve as a key to his argument: and this seems the more necessary, as he has been thought by some (9) to

(9) So obscurely has the best of all the antient writers (Ptolemey) delivered himself on this article (the tones), that deserved to have been most clearly handled. Malcolm's Treatise on Music, p. 539.
have

have written very obscurely upon the subject. His chief view was to reduce the fifteen modes, admitted upon the principle of the harmonic doctrine, to those seven, which had the support also of the musical: but this reformation was to be attempted with great precaution, on account of the obstinate prejudices it was likely to meet with, from the harmonicians of his own time: And we shall therefore find him first artfully treating the modes upon the foot of the harmonic doctrine only, and arguing *ad homines* for the reduction of the modes, even upon their own principles; but, before he drops the subject, his true reasons will appear; and, indeed, it will easily be seen, that he had them constantly in view from the first, though he does not argue openly upon them. This will account for that mixture of the two doctrines, which is found in his writings, and will, with the assistance of a few explanations, render intelligible what he has delivered upon the subject.

The tones he considers, after the harmonic doctrine, as mutations by whole constitutions, which, he tells us, are therefore properly called tones, as they are differences in respect to tension. These differences he admits to be infinite in possibility, but argues, that in efficacy, and in respect to sense, they are finite, and liable to certain limitations. These limitations he derives from the theory of the consonances, by which means, he lays a foundation for his future argument, for reducing the modes to the number of species of the consonance diapason. The limitations of the tones, which he proposes to regulate by those of the consonances, are, as he tells us, threefold; *viz.*

1. As to the distance, or ratio, as he expresses it, of the extreme tones; for the Pythagoreans, whose method he adopts with some improvements of his own, measured intervals by the ratios of the comprehending sounds.

2. As to the number of tones to be admitted between these extremes. And,

3. As to the intervals, at which they were to stand in succession, which he calls their excesses.

Thus in the diatessaron consonance, which he instances, these three circumstances are observable; first, That the ratio of the extreme sounds is sesquitercian; secondly, That the component intervals, or ratios, are three; and thirdly, That such and such are the differences of those ratios, meaning the intervals in succession. But here he observes, that, in the consonances, these limitations have each their distinct cause; whereas in the tones, the first being determined, the other two necessarily followed, as being dependent on the same conditions. This remark will hardly be intelligible, without some explanation. The interval, or ratio of the extreme sounds in each consonance, though differently treated by the Aristoxenian and Pythagorean schools, were yet determined, both by the one and the other, upon principles, which concerned not their intermediate division: their intermediate division again was settled by a doctrine, that required, in the composition of intervals, either that every fourth sound should complete the diatessaron, or every fifth the diapente; without one or other of which circumstances, the composition was held inconcin-

nous

nous (10); and hence the diateffaron could not be divided into more than three ratios, nor the diapente into more than four; and again, the excesses of the ratios, or intervals in succession, were assigned by the doctrine of the genera; so that in the consonances, each of the three limitations had a separate cause; but in applying the same limitations to the tones, there was a connection between them; so that the determining the first, determined the rest. Thus, for instance, if the interval diateffaron, or sequitertian ratio, should be assigned for the limitation of the extreme tones, the other limitations could only be such, as were assigned for that consonance; that is, the component ratios must be three, and the excesses of those ratios such, as were established for the diateffaron by the doctrine of the genera. But the not knowing the necessity of this consequence, he tells us, had occasioned a great disagreement amongst the harmonicians in determining these points, some making the interval of the extreme tones less than a diapason, others equal to it, and others again exceeding it; the later harmonicians, in particular, being fond of adding something to what had been admitted by those, who went before them. To bring these various opinions to some determination, he proposes the restitution of similitude in the hermosthenon, or harmonised melos, as the chief circumstance, by which to regulate the

(10) Ἐν παντὶ δὲ γένει ἀπὸ παντὸς οδοῦ διὰ τῶν ἐξῆς τὸ μέλος ἀγόμενον, καὶ ἐπὶ τὸ βαρὺ, καὶ ἐπὶ τὸ ἥϋλ, ἢ τὸν τέταρτον τὸ ἐξῆς διατεσσάραν, ἢ τὸν πέμπτον διασπέντε σύμφωνον λαμβάνεται. ὃ δ' ἂν μετέρεα τῶτων συμβαίνει, ἐκτελής ἐς ὧς πρὸς ἅπαντας, ἐν οἷς συμβέβηκεν ἀσυμφώνῳ εἶναι κατὰ τὰς εἰρημένους ἀειθμὰς. Aristox. p. 54. edit. Meibom.

first limitation; and this, he says, is effected by the diapason, for, at that distance, there was always a return of similar harmony; so that the tone, that was a diapason distant from that first taken, was a repetition of it; and the tone still farther distant, as for instance, at a diapason and diatessaron, the same with the tone distant a diatessaron only, and so of the rest. For a proof of this, he appeals to the practice of music; where it was well known, that in such mutations, as were at the interval of a diapason, no one of the strings required new tuning, though in all other mutations some alterations were necessary. And hence he concludes, that those, who make the distance of the extreme tones less than the diapason, do not restitute the *hermosmenon*, there being still other tones beyond the limit they assign, unlike to those before taken; and that those, who exceed the diapason, admit redundancies, by repeating some of the harmonies before taken; and further insists, that even they, who proceeded no farther than the diapason, were yet to blame, inasmuch as they took in the tone, that was a diapason distant from the first, since, in so doing, they admitted one redundancy, and thereby gave a handle to those, whom they charged with passing the proper limits for the modes, to accuse them, in turn, with being really the authors of this licence, since, if one useless tone be admitted, the same privilege may be fairly claimed for a greater number. This last argument seems levelled at the Aristoxenians, who admitted no more than thirteen modes; because they would not exceed the diapason; but whose Hyperphrygian was, according to this argument, a repetition of the Hypodorian. The
author

author supports this argument still farther, by an appeal to the circumstance of the species of diapason, the sounds of which were eight, but the number of the species seven only, answering not to the number of terms in the division, but to that of the component ratios: for that the diapason taken from the gravest sound towards the grave, yielded the same species with the first diapason taken from the acuteest sound towards the same parts, was out of dispute, it holding true universally, that whatever takes its beginning in the same manner from either of the extremes of the diapason, produces the same power. And here he leaves the first limitation, without expressly assigning the interval for the extreme tones; for the title of the chapter, which seems to fix it to a diapason, ought to be understood only in this sense, that it should not exceed it; which agrees with the reasoning in the chapter itself. As to the conclusion, which depended on the two other limitations, if I may venture to draw it for him, it will stand thus, that into what number soever of terms the diapason be divided, the distance for the extreme tones should be the interval between the first term and the last but one.

The first limitation being thus far considered, he proceeds to determine the next, upon which depended the number of the tones; and here he again opposes the Aristoxenians, rejecting, by his theory of this limitation, five of their thirteen modes, besides the Hyperphrygian, which stood condemned by the former one, and leaving only seven, according to the number of the species of diapason, which he proposes as the properest rule, by which to govern this limitation; and assigns for this the following reasons.

The

The number of the species of diapason was equal to that of the species of the two first consonances taken together, and the species of these last were taken according to the condition of the ratios in each, the number of which the very nature of them would not permit to be either increased or diminished. Now the tones contained within the diapason following the nature of the consonances, and being indeed established on their account, *viz.* that the whole systems might have consonant differences, he argues, that those, who were either for admitting more than seven, which was the number both of the species and of the ratios in the diapason, or for making all the excesses of the tones equal, were not to be assented to, since they could not assign any satisfactory reason, either for the equality of the increments in general, which, in the harmonic genus, was particularly inconvenient, or for fixing either on the tone, hemitone, or diesis, in particular, for the common excess, (from the supposition of one or other of which, they determined the number of tones, according to the number of such intervals contained within the diapason). For what was there to determine such a preference, when the consonance (meaning the diapason) was, as they themselves allowed, susceptible not only of all these excesses, but of many others, in the orders both of the genera and of the distances? Nor could they say, that such a magnitude divides the diapason exactly, and such another not exactly, or one, perhaps, into an even number of parts, and another into an uneven: for though the diapason was divided into six by the tone, into twelve by the hemitone, into eighteen by the third of the tone, and into twenty-four by the quarter,

quarter; yet, in all these divisions, there was a variation from the exact truth, even perceptible to the ear. All this reasoning is again manifestly levelled at the Aristoxenians, who, falsely esteeming the diapason to consist exactly of six tones, or twelve hemitones, had admitted thirteen modes, at the distance of a semitone from each other: and here we see the author still combats them fairly on the principles of the harmonic doctrine. But, to go on with his arguments; having thus fixed the number of the modes to seven only, he proceeds to consider the third limitation. It will be asked, says he, what are to be the excesses chosen for these seven modes, since the diapason cannot be divided into seven equal parts; and, if unequal intervals be admitted, there is still room for variety in the choice? In answer to this, he proposes, that the intervals in question shall be taken by means of the first consonances, diatessaron and diapente, and their differences or excesses, whatsoever they may be: for he argues, that the mutations, by consonant intervals, ought to be first established; and next to these, the mutations, by concinnous intervals. Those, who admitted eight tones, he tells us, had found their positions after the following manner. The Dorian, Phrygian, and Lydian, the three most antient, being first set at the distance of a tone from each other, they made the next mutation consonant, at a diatessaron from the Dorian, towards the acute, and at a limma from the Lydian, calling it Mixolydian; because it lay not at the distance of a tone from the Lydian, but only at the distance of a limma, or difference between ditonus and diatessaron; then this new mode having
the

the Dorian graver than it by a diatessaron, that the rest of the four might be attended with the like circumstance, they established the Hypolydian, Hypophrygian, and Hypodorian, a diatessaron graver respectively than the Lydian, Phrygian, and Dorian; and lastly, they placed the Hypermixolydian at a diapason from the Hypodorian, towards the acute, giving it that denomination from its position above the Mixolydian, misusing, as he tells us, the preposition *hypo* to signify the grave, and *hyper* to express the acute. But though the result of this method is the same with that of his own, excepting as to the Hypermixolydian, (which he excludes for the reason assigned in treating of the first limitation) yet he objects to it; because the concinnous intervals are first assumed, which ought to be taken by the consonances; and gives his own, which is by the continual addition of the consonance diatessaron, but, however, within the compass of the diapason. Wherefore, when such addition would exceed it, he directs taking the diapente in its stead, the contrary way; whereby the division will be kept within the limits assigned. This method, and its result, will perhaps be better understood, and will also be more succinctly couched in our modern expressions, in which I shall therefore give it, applying to it the corresponding characters of the Guidonian scale.

Having first taken the Mixolydian tone, which was the acutest of the seven, suppose in D, fall a fourth to A, for the Dorian; another fourth to E, for the Hypodorian; then, to avoid passing the bounds of the diapason, rise a fifth to B, for the Phrygian; fall a fourth again to *f*#, for the Hypo-
phrygian;

phrygian; rise a fifth to $c^\sharp$, for the Lydian; and fall a fourth to $G^\sharp$, for the Hypolydian.

By this method, the positions of the seven modes come out exactly, as I fixed them from Bacchius, in explaining the harmonic doctrine; and we see, that, for settling them, Ptolemy has really recourse to no other theory of the modes, than that admitted by those he contends with, though he makes the species of the consonances, and those of the diapason more particularly, the governing rule for fixing their positions, as the only means, by which the two doctrines could be made to coincide. But it remained, after thus settling the seven modes, to shew more fully the consequence of following the method of the Aristoxenians, and others, who divided the tonic spaces found by his method, and placed the modes in a semitonic succession, by which their number had been raised to thirteen, even within the compass of the diapason; and, in doing this, we shall find he ventures to assign the true reason for his reduction, which was grounded on the musical doctrine. This argument, which is contained in the eleventh chapter of his second book, being very remarkable, and seeming strongly to support the combination of the two doctrines in the diagram I have given of the seven modes, I shall give a translation of the whole chapter, lest I should be thought to strain his arguments in favour of the musical doctrine, which has been thought by many to have little or no relation to the modes, and which, if we except what this author has delivered, seems indeed, upon a slight examination, and comparison of the evidence, to have the weaker support.

Now, these being the tones we have established, it is plain, that a certain sound of the diapason is appropriated to the mese, in power of each, by reason of their being equal in number to the species. For a diapason being selected out of the middle parts of the perfect system; that is, the parts from hypate meson in position to nete diezeugmenon, (because the voice is most pleased to be exercised about the middle melodies, seldom running to the extremes, because of the difficulty and constraint in immoderate intentions and remissions), the mese in power of the Mixolydian will be fitted to the place of paranete diezeugmenon, that the tone may, in this diapason, make the first species; that of the Lydian, to the place of trite diezeugmenon, according to the second species; that of the Phrygian, to the place of paramese, according to the third species; that of the Dorian, to the place of the mese, making the fourth and middle spaces of the diapason; that of the Hypolydian, to the place of lichanos meson, according to the fifth species; that of the Hypophrygian, to the place of parhypate meson, according to the sixth species; that of the Hypodorian, to the place of hypate meson, according to the seventh species; that so it may be possible, in the alterations required for the tones, to keep some of the sounds of the system unmoved, for preserving the magnitude of the voice (meaning the pitch of the diapason), it being impossible for the same powers, in different tones, to fall upon the places of the same sounds. But, should we admit more tones than these, as they do, who augment their excesses by hemitones, the meses of two tones must, of necessity, be applied to the place of one sound; so that,

in

in interchanging the tunings of those two tones, the whole system in each must be removed, not preserving any one of the preceding tensions in common, by which to regulate the proper [pitch] of the voice. For the mese in power of the Hypodorian, for instance, being fixed to hypate meson by position, and that of the Hypophrygian to parhypate meson, the tone taken between these two, and called by them the graver Hypophrygian, to distinguish it from the other acuter one, must have its mese either in hypate, as the Hypodorian, or in parhypate, as the acuter Hypophrygian; which being the case, when we interchange the tuning of two such tones, which use one common sound, this sound is indeed altered a hemitone in pitch, by intention or remission; but having the same power in each of the tones, *viz.* that of the mese, all the rest of the sounds are intended or remitted in like manner, for the sake of preserving the ratios to the mese, the same with those taken before the mutation, according to the genus common to both tones; so that this tone is not to be held different in species from the former, but the Hypodorian again, or the same Hypophrygian, only somewhat acuter or graver in its pitch. That these seven tones, therefore, are sufficient, and such as the ratios require, be it thus far declared (11).

With

(11) Δῆλον δὲ ὅτι, καὶ τούτων μὲν ὑποθεμένων ἡμῖν τῶν τόνων, τῆς καθ' ἑκάστον τῇ δυνάμει μέσης, ἰδίως τις γίνεσθαι τῷ διὰ πασῶν φθόγῳ, διὰ τὸ ἰσάειδμον αὐτῶν τε καὶ τῶν εἰδῶν. Ἐκλαμβάνομεν γὰρ τῷ διὰ πασῶν κατὰ τὰς μετὰξὺ πῶς τῷ τελείῳ συστήματι τῶν πεντηκόντων, τὰς ἀπὸ τῆς θέσεως τῶν μεσῶν ὑπάτης ἐπὶ τὴν νήτην διεξελγμένων ἕνεκα τῷ τὴν φωνὴν ἐμφιλοχῶρως ἀνασφραῖσθαι καὶ καθαρῶς γίνεσθαι περὶ τὰς μέσας μάλιστα μελωδίας ὀλιγάκις ἐπὶ τὰς ἀκρας ἐκβαίνου-

With this chapter he ends his doctrine of the modes, and of their reduction; and we see he here makes the musical doctrine of the species the basis of his theory, as far as concerns the rejecting the eight redundant modes; though, out of caution, and to obviate objections, he had established the seven upon the harmonic doctrine of the pitch of the

σαν, διὰ τὸ τῆς παρὰ τὸ μέτερον χαλάσεως ἢ κατὰτάσεως ἐπίπονον καὶ βεβιασμένον. Ἡ μὲν τῆ μιζολυδία μέση κατὰ τὴν δύναμιν, ἐφαρμόζεται τῷ τόπῳ τῆς παρηνήτης τῶν διεζευγμένων ἴν' ὁ τόνος τὸ πρῶτον εἶδος, ἐν τῷ περὶ κείμενα, ποιήσῃ τῆ διὰ πασῶν. Ἡ δὲ τῆ λυδία, τῷ τόπῳ τῆς τρίτης τῶν διεζευγμένων, κατὰ τὸ δεύτερον εἶδος. Ἡ δὲ τῆ φρυγίας, τῷ τόπῳ τῆς παρμεσίας, κατὰ τὸ τρίτον εἶδος. Ἡ δὲ τῆ δωείας, τῷ τόπῳ τῆς μέσης, ποιήσῃ τὸ τέταρτον καὶ μέσον εἶδος τῆ διὰ πασῶν. Ἡ δὲ τῆ ὑπολυδίας, τῷ τόπῳ τῆς λιχανῶ τῶν μέσων, κατὰ τὸ πέμπτον εἶδος. Ἡ δὲ τῆ ὑποφρυγίας, τῷ τόπῳ τῆς παρυπάτης τῶν μέσων, κατὰ τὸ ἕκτον εἶδος. Ἡ δὲ τῆ ὑποδωείας, τῷ τόπῳ τῆς τῶν μέσων ὑπάτης, κατὰ τὸ ἕβδομον εἶδος. *Ὡς τε δύνασθαι τινὰς ἐν τῷ συστήματι τηρεῖσθαι φθόγους ἀκινήτους, ἐν ταῖς τῶν τόνων μεθαρμογαῖς, παραφυλάσσοντας τὸ μέγεθος τῆς φωνῆς διὰ τὸ μηδέποτε τὰς ἐν διαφορῇ τόνοις ὁμοίας δυνάμεις, τοῖς ἢ αὐτῶν φθόγων τόποις περικεῖσθαι. Πλεόνων δὲ τῶν τόνων παρὰ τέτταρας ὑποτιθεμένων ὁ ποιῶσιν οἱ ἐν τοῖς ἡμιτονίοις τὰς ὑπερχᾶς αὐτῶν παρὰ ὑψοῖς ἀναγκαῖον εἶναι, δύο τόνων μέσας ἐνδὸς φθόγου τόπῳ παντὶ ἐφαρμόξεν. ὥς δ' ὅλα κινῆσθαι τὰ συστήματα, κατὰ τὴν εἰς ἀλλήλους τῶν δύο τέτων τόνων μεθαρμογήν· μηκέτι τηρεῖν κοινὴν τινὰ τὴν ἐξ ἀρχῆς τάσιν, ἢ παραμεινῆσθαι τὸ ἴδιον τῆς φωνῆς. Τῆς μὲν γὰρ τῆ ὑποδωείας, φέρεται εἶναι, τῇ δυνάμει μέσης, συνεζευγμένης τῇ κατὰ τὴν θέσιν τῶν μέσων ὑπάτη· ἡ δὲ τῆ ὑποφρυγίας τῇ τῶν μέσων παρυπάτη τὸν λαμβανόμενον μέγιστον τέτων τόνον, καλέμενον δὲ ὑπὸ αὐτῶν βαρύτερον ὑποφρύγιον, παρ' ἐκείνον, ὀξύτερον, δεήσει τὴν αὐτὴν μέσιν, ἥτοι κατὰ τὴν ὑπάτην ἔχειν, ὥς καὶ ὁ ὑποδωείας ἢ κατὰ τὴν παρυπάτην, ὥς καὶ ὁ ὀξύτερος ὑποφρύγιος. Οὐ συμβαίνειν δὲ, ἐπειδὴν εἰς ἀλλήλους μεθαρροζώμεθα τὰς τὴν κοινὸν φθόγον ἐπιφύτας τόνους· κινηθῆσθαι μὲν ἕτερον ἐπιφύτας ἢ χαλαροῦς ἡμιτονίῳ· τῷ δὲ τὴν αὐτὴν, ἐν ἑκατέρῳ ἢ τόνῳ, δύναμιν ἔχειν τετάρτην ἢ τὴν μέσιν· ἀκολουθήσασιν αἱ ἢ λοιπῶν ἀπείρων φθόγων ἐπιτάσεις ἢ χαλάσεις· ἐνεκα τῆ συνηρῆν τὰς πρὸς τὴν μέσιν λόγους, τὰς αὐτὰς τοῖς πρὸ τῆ μεταβολῆς, κατὰ τὸ κοινὸν ἀμφοτέρων ἢ τῶν τόνων γένος, λαμβανόμενοις. *Ὡς τε μὴδ' ἂν ἕτερον ἔτι δόξαι τῷ εἶδει τὴν τόνον πρὸς τὴν ἁρότερον· ἀλλ' ὑποδωείον παλιν, ἢ τὴν αὐτὸν ὑποφρύγιον, ὀξυφωνότερον τινὸς ἢ βαρυφωνότερον μίον. Τὸ μὲν ἔν' εὐλογόν τε καὶ ἀνιάρχους ἢ ἐπὶ τῶν τόνων, μεχεῖ τέτων ὑποφρυγίων. Ptol. lib. ii. c. 11.

system.

system. That the force of his arguments in this chapter, and the result of admitting the eight modes, may be more plainly seen, I have annexed a second diagram of the species, as they lay in the spurious modes; by which it will appear, to which species in the genuine ones they severally answered; and the objection, raised against each of them by Ptolemey will become intelligible.

There is, however, an objection, to which this second diagram seems liable. It may be asked, if the Hypoionian mode, instanced by Ptolemey, could give either Hypodorian or Hypophrygian harmony, according as either the eighth or the ninth string was employed for its mese? and if the rest of the spurious modes were liable to a like alternative, what was it determined me in my choice of the two species, in the construction of the diagram? To which I answer, that the antients have decided for me as to five of them; for we find (12), that the Hypoionian was called the graver Hypophrygian; the Hypoæolian, the graver Hypolydian; the Ionian, the graver Phrygian; the Æolian, the graver Lydian; and the Hyperionian, the acuter Mixolydian. And this, by the way, furnishes us with an unanswerable argument of the antiquity of the doctrine enforced by Ptolemey, that the number of the modes ought not to exceed that of the species of the diapason; for these appellations, which were given to the modes long enough before Ptolemey flourished, can be accounted for on no other principle, but the supposing the species of

(12) Vide Euclid. *Introduct. Harm.* p. 19. v. 29. & seq. *Arist. Quint.* p. 23. v. 7. & seq.

diapason to have been considered as the essential characters of the modes; and that these five modes therefore, though they had a difference in pitch, were thought to be the same, as to their effects, with those, after which they were thus named. As to the other three, the Hyperphrygian, Hyperæolian, and Hyperlydian, their meses being respectively at the distance of an octave from the Hypodorian, Hypoionian, and Hypophrygian, it was necessary to give them the same species with those three modes; for a transposition of an octave was always understood to give a return of the same harmony, as has been already shewn.

This second diagram, and that of the seven genuine modes, may be thought sufficient to render the subject intelligible: however, as the fifteen modes are parted in the two diagrams, and confined also to the compass of a diapason, for shewing the species, I have added a table, shewing the tuning of the fifteen strings of the lyre for all the modes. The letters of Guido's scale, placed within the squares, shew, in their horizontal orders, the tunings of all the strings for each mode; the two broader lines include the species of diapason; and the meses are distinguished by capital letters, by which means, the cases, where the same string is employed for the meses of two modes, appear at the first view; and the reasoning, upon which Ptolemey rejects the eight modes, is illustrated, so as to need no farther explanation.

Having now gone through with what I proposed to explain under the three first heads, I come,

Fourthly,

A TABLE, shewing the Tuning of the STRINGS of the LYRE for every Mode.

STRINGS of the LYRE.

		Hyperlydian.	Hyperæolian.	Hyperphrygian.	Hyperionian.	Hypæolian, or Mixolydian.	Lydian.	Æolian.	Phrygian.	Ionian.	Dorian.	Hypolydian.	Hypoæolian.	Hypophrygian.	Hypoionian.	Hypodorian.
15	Nete hyperbolæon.	<i>a</i>	<i>a^b</i>	<i>a</i>	<i>b^b</i>	<i>a</i>	<i>a</i>	<i>a^b</i>	<i>a</i>	<i>a^b</i>	<i>a</i>	<i>a[#]</i>	<i>a</i>	<i>a</i>	<i>a^b</i>	<i>a</i>
14	Paranete hyperbolæon.	<i>g[#]</i>	<i>g</i>	<i>g</i>	<i>a^b</i>	<i>g</i>	<i>g[#]</i>	<i>g</i>	<i>g</i>	<i>g^b</i>	<i>g</i>	<i>g[#]</i>	<i>g</i>	<i>g[#]</i>	<i>g</i>	<i>g</i>
13	Trite hyperbolæon.	<i>F[#]</i>	<i>F</i>	<i>f[#]</i>	<i>g^b</i>	<i>f</i>	<i>f[#]</i>	<i>f</i>	<i>f[#]</i>	<i>f</i>	<i>f</i>	<i>f[#]</i>	<i>f</i>	<i>f[#]</i>	<i>f</i>	<i>f[#]</i>
12	Nete diezeugmenon.	<i>e</i>	<i>e^b</i>	<i>E</i>	<i>f</i>	<i>e</i>	<i>e</i>	<i>e^b</i>	<i>e</i>	<i>e^b</i>	<i>e</i>	<i>e</i>	<i>e^b</i>	<i>e</i>	<i>e^b</i>	<i>e</i>
11	Paranete diezeugmenon.	<i>d</i>	<i>d^b</i>	<i>d</i>	<i>E^b</i>	<i>D</i>	<i>d[#]</i>	<i>d</i>	<i>d</i>	<i>d^b</i>	<i>d</i>	<i>d[#]</i>	<i>d</i>	<i>d</i>	<i>d^b</i>	<i>d</i>
10	Trite diezeugmenon.	<i>c[#]</i>	<i>c</i>	<i>c</i>	<i>d^b</i>	<i>c</i>	<i>C[#]</i>	<i>C</i>	<i>c[#]</i>	<i>c</i>	<i>c</i>	<i>c[#]</i>	<i>c</i>	<i>c[#]</i>	<i>c</i>	<i>c</i>
9	Paramese.	<i>b</i>	<i>b^b</i>	<i>b</i>	<i>c^b</i>	<i>b^b</i>	<i>b</i>	<i>b^b</i>	<i>B</i>	<i>B^b</i>	<i>b</i>	<i>b</i>	<i>b^b</i>	<i>b</i>	<i>b^b</i>	<i>b</i>
8	Mese.	<i>a</i>	<i>a^b</i>	<i>a</i>	<i>b^b</i>	<i>a</i>	<i>a</i>	<i>a^b</i>	<i>a</i>	<i>a^b</i>	<i>A</i>	<i>a[#]</i>	<i>a</i>	<i>a</i>	<i>a^b</i>	<i>a</i>
7	Lichanos meson.	<i>g[#]</i>	<i>g</i>	<i>g</i>	<i>a^b</i>	<i>g</i>	<i>g[#]</i>	<i>g</i>	<i>g</i>	<i>g^b</i>	<i>g</i>	<i>G[#]</i>	<i>G</i>	<i>g[#]</i>	<i>g</i>	<i>g</i>
6	Parhypate meson.	<i>f[#]</i>	<i>f</i>	<i>f[#]</i>	<i>g^b</i>	<i>f</i>	<i>f[#]</i>	<i>f</i>	<i>f[#]</i>	<i>f</i>	<i>f</i>	<i>f[#]</i>	<i>f</i>	<i>F[#]</i>	<i>F</i>	<i>f[#]</i>
5	Hypate meson.	<i>e</i>	<i>e^b</i>	<i>e</i>	<i>f</i>	<i>e</i>	<i>e</i>	<i>e^b</i>	<i>e</i>	<i>e^b</i>	<i>e</i>	<i>e</i>	<i>e^b</i>	<i>e</i>	<i>e^b</i>	<i>E</i>
4	Lichanos hypaton.	<i>d</i>	<i>d^b</i>	<i>d</i>	<i>e^b</i>	<i>d</i>	<i>d[#]</i>	<i>d</i>	<i>d</i>	<i>d^b</i>	<i>d</i>	<i>d[#]</i>	<i>d</i>	<i>d</i>	<i>d^b</i>	<i>d</i>
3	Parhypate hypaton.	<i>c[#]</i>	<i>c</i>	<i>c</i>	<i>d^b</i>	<i>c</i>	<i>c[#]</i>	<i>c</i>	<i>c[#]</i>	<i>c</i>	<i>c</i>	<i>c[#]</i>	<i>c</i>	<i>c[#]</i>	<i>c</i>	<i>c</i>
2	Hypate hypaton.	<i>b</i>	<i>b^b</i>	<i>b</i>	<i>c^b</i>	<i>b^b</i>	<i>b</i>	<i>b^b</i>	<i>b</i>	<i>b^b</i>	<i>b</i>	<i>b</i>	<i>b^b</i>	<i>b</i>	<i>b^b</i>	<i>b</i>
1	Proslambanomenos.	<i>a</i>	<i>a^b</i>	<i>a</i>	<i>b^b</i>	<i>a</i>	<i>a</i>	<i>a^b</i>	<i>a</i>	<i>a^b</i>	<i>a</i>	<i>a[#]</i>	<i>a</i>	<i>a</i>	<i>a^b</i>	<i>a</i>



DIAGRAM II. Of the Species of Diapason in the Eight Modes rejected by PTOLEMEY.

Hyperlydian M. Hypophrygian Sp.		Hyperæolian M. Hypophrygian Sp.		Hyperphrygian M. Hypodorian Sp.		f Paramefe. Hyperionian M. Diaz. tone. Mixolydian Sp. Mefe.		e ^b Trite diez. Æolian M. Lydian Sp. Mefe.		e ^b Paran. diez. Ionian M. Phrygian Sp. Mefe.		e ^b Trite hyp. Hypoæolian M. Hypolydian Sp. Mefe.		e ^b Paran. hyp. Hypoionian M. Hypophrygian Sp. Mefe.	
e	Lich. mef.	e ^b	Lich. mef.	e	Mefe.	f	Paramefe. Diaz. tone. Mefe.	e ^b	Trite diez.	e ^b	Paran. diez.	e ^b	Trite hyp.	e ^b	Paran. hyp.
d	Parhyp. mef.	d ^b	Parhyp. mef.	d	Lich. mef.	d ^b	Lich. mef.	d	Paramefe. Diaz. tone. Mefe.	d ^b	Trite diez.	d	Nete diez.	d ^b	Trite hyp.
c [#]	Hyp. mef.	c	Hyp. mef.	c	Parhyp. mef.	c ^b	Parhyp. mef.	c	Mefe.	c	Paramefe. Diaz. tone. Mefe.	c	Paran. diez.	c	Nete diez.
b	Lich. hyp.	b ^b	Lich. hyp.	b	Hyp. mef.	b ^b	Parhyp. mef.	b ^b	Lich. mef.	b ^b	Diaz. tone. Mefe.	b ^b	Trite diez.	b ^b	Paran. diez.
a	Parhyp. hyp.	a ^b	Parhyp. hyp.	a	Lich. hyp.	a ^b	Lich. hyp.	a ^b	Parhyp. mef.	a ^b	Lich. mef.	a	Paramefe. Diaz. tone. Mefe.	a ^b	Trite diez.
g [#]	Hyp. hyp.	g	Hyp. hyp.	g	Parhyp. hyp.	g ^b	Parhyp. hyp.	g	Hyp. mef.	g ^b	Parhyp. mef.	g	Mefe.	g	Paramefe.
f [#]	Proflamb.	f	Proflamb.	f [#]	Hyp. hyp.	f	Parhyp. hyp.	f	Lich. hyp.	f	Hyp. mef.	f	Lich. mef.	f	Diaz. tone. Mefe.
e	Paran. hyper.	e ^b	Paran. hyperb.	e	Proflamb.	e	Hyp. hyp.	e ^b	Parhyp. hyp.	e ^b	Lich. mef.	e ^b	Parhyp. mef.	e ^b	Lich. mef.



Fourthly, To point out something of the origin of the two doctrines.

The harmonic doctrine was, as I have shewn, expressly taught in harmonic; and therefore, to account for its reception, it will be necessary to consider the scope of that science.

The musical doctrine I have also shewn to have been explained in harmonic, as far as concerned the elementary principles of the species of diapason; but the application of those principles to the doctrine of the modes was not taught therein, being reserved for melopœia, a science, that looked higher than harmonic, and considered the use of the elements, when in the hands of the poet or musician. It is to this science, therefore, that we must look for the origin of the musical doctrine.

But, before I consider the scope of either of these sciences, it will be necessary to shew my reasons for separating them, since they were blended by the Aristoxenians, as has been shewn above. For this purpose, we must have recourse to the division of music, the mother science; which, as treated by the antients, comprehended all, that the moderns now understand by music and poetry. Its first general division, according to Aristides Quintilianus (13), was into theoretic and practick. The theoretic was subdivided into natural and artificial; the latter of which was again subdivided into harmonic, rhythmic, and metric;

(13) De Musica, p. 7. See also Meibomius on the passage, who has embraced the division of Aristides, in the following table.

metric; which three sciences more particularly contained the poets elements, as teaching the grounds of tune, time, and verse. But nothing less than a competent knowlege of every branch of the mother science could carry with it the respected title of *μουσικος*, the musician. To harmonic, rhythmic, and metric, in the theoretic, respectively answered melopœia, rhythmopœia, and poetic, in the practic. In this author's division therefore, we see, that harmonic and melopœia are distinguished, even by the first general division. Of the propriety of this even the Aristoxenians seem to have been sensible; since, in order to justify their blending them, they have defined harmonic as a science, both theoretic and practic (14). And Aristoxenus himself, in the fragments we have under the false title of his three books of harmonic elements, seems to assign such bounds to harmonic, as might well be understood to exclude

MUSICÆ	{	alia est pars	{	Phyfica,	{	Arithmetica,
		Theoretica,		quæ dividitur in		Phyficam generi cognominem.
	{	cujus rursus		Artificialis,	{	Harmonicam,
		partes duæ		quæ dividitur in		Rhythmicam,
						Metricam.
	{	alia	{	Usualis,	{	Melopœia,
		Practica,		cujus partes		Rhythmopœia,
	{	cujus item		Enarrativa,	{	Poesis.
		partes duæ		cujus partes		Organica,
						Odica,
						Hypocritica.

(14) Ἀρμονική ἐστὶν ἐπιστήμη θεωρητικὴ καὶ πραγματικὴ τῆς ἡρμωσμένης οὐσίας. Euclid. Introd. Harmon. initio.

melopœia

melopœia (15). We have Plutarch also on our side, who, in a passage of his dialogue on music, which I shall have occasion to cite, amongst other proofs, enumerates only the first six parts of harmoniac, leaving melopœia out of his division. For this Meibomius blames him (16), but too hastily; for had the learned critic considered the whole passage, he must have seen, that the omission was not through ignorance, but design, the author being there expressly arguing for the confining of harmonic within its proper bounds. However, whether the Aristoxenians were right or wrong, in including melopœia in harmonic, my argument seems not materially affected by it; for they gave it only as a last division, to be taught after all the rest had been inculcated; and they agreed in defining it to be the use of the harmonic elements, according to the propriety of each subject (17): which is distinction enough for the use I propose to make of it.

Having thus far justified my dividing these sciences, I shall now consider the scope of each; which

(15) Τῆς περὶ μέλους ἐπισήμης πολυμερῆς ἔσης, καὶ διηρημένης εἰς πλείους ἰδέας, μίαν τινὰ αὐτῇ ὑπολαβεῖν δεῖ, τὴν ἁρμονικὴν καλεμένην, εἶναι πραγματεῖαν, τῇ τε τάξει πρῶτην ἔσαν, ἔχουσάν τε δύναμιν σοιχεώδη. Τυγχάνει γὰρ ἔσα πρῶτη τῇ θεωρητικῶν· τὰυτὰ δὲ εἰς ὅσα συντείνει πρὸς τὰ τῶν συστημάτων τε καὶ τόνων θεωρίαν. Περσῆκει γὰρ μηδὲν πορρωτέρω τέτων ἀξιῶν παρ' αὐτῆς τῆς εἰρημένην ἐχούσης ὁπσιμὴν τέλει γὰρ τὰτὰ εἰς τὴν πραγματείας ταύτης. Aristox. p. 1. init.

(16) Plutarchus dialogo de Musica sex tantum priores turbato etiam ordine recenset, cum tamen crebram μελοποιίας mentionem injiciat, adeoque, non una ratione excusari illud potest. Not. in Gaudientium, p. 30.

(17) Μελοποιία δὲ εἰς χρῆσιν ἢ ὑποκειμένων τῇ ἁρμονικῇ πραγματείας, πρὸς τὸ οἰκτὸν ἐκείνης ὑποθέσεως. Euclid. Introd. Harmon. p. 2. v. 5. & p. 22. v. 18.

will lead us to the origin of the doctrines they contained.

The object of *melopœia* was propriety in the use of the harmonic elements, as appears from the definition just cited. In what this propriety consisted, it is difficult to know, as no collection of the rules of this science is come down to us: it is not to be learnt from the harmonic writers, who treat very slightly of this branch; and those of them, who are the fullest upon it, as Aristides Quintilianus and Manuel Briennius, wrote in later times, when the fashion of music was changed, and a new kind of *melopœia* prevailed, with the rules of which the earlier musicians could hardly have been acquainted; as, on the other hand, many of the earlier rules must have been forgot, or, in a great measure, disused. There is more light to be procured from the dialogue of Plutarch, and some others, who speak occasionally of the practice of music. I do not propose to collect any thing here upon this subject, as, indeed, there does not seem to be sufficient materials left us for a thorough explanation of it, which would require no less than a complete history of the science of its music, and its improvements: it is sufficient for my purpose to say, that, in the earlier times, when the best music is supposed to have prevailed, the genera, modes, *rhythmi*, metres, &c. were not employed indiscriminately, nor even left to the discretion of the musician, but were under particular restrictions, which confined them to the arguments, to which they were held respectively the best adapted; and that it was from the mixture of these ingredients, that arose the ethic character of the composition. Hence the several
nomi

nomi took their rise; of which there is, in the dialogue of Plutarch, an ample account. As what I am now advancing cannot but be well known, I need look no farther for the proof of it, than to a passage of Plato's third book of laws, where he complains of a licence beginning even in his time to the prejudice of the science. Speaking of times past,

Our music (says he) was then divided according to certain species, and figures thereof. Prayers to the gods were one species of song, to which they gave the name of hymns: opposed to this was another species, which, in particular, might be called threni; another, pæones; and another, the birth of Dionysius, which I hold to be the dithyrambus: there were also citharædic nomi, so called, as being still another song. These, and some others, being prescribed, it was not allowable to use one species of melos for another.—But afterwards, in process of time, the poets first introduced an unlearned licence, being poetic by nature, but unskilled in the rules of the science, trampling upon its laws, over attentive to please, mixing the threni with the hymns, and the pæones with the dithyrambi, imitating the music of the flute upon the cithara, and confounding all things with all, &c. (18).

This

(18) Διηρημένη γὰρ τότε ἦν ἡμῶν ἡ μουσικὴ κατ' εἶδη τε αὐτὴ ἅπλῃ καὶ χήμασι· καὶ τί ἦν εἶδος ὧδ' ἡς εὐχαὶ πρὸς θεοὺς· ὅρομα δὲ ὕμνοι ἐπικάλυπτοι· καὶ τέτρα, δὲ τὸ ἐναντίον ἦν ὧδ' ἡς ἕτερον εἶδος· θρήνης δὲ τίς αὖν αὐτὰς μάλιστα ἐκάλεσε· καὶ παιῶνες, ἕτερον καὶ ἄλλο Διονύσου γένεσις, οἷμαι, διθύραμβος· λεγόμενος νόμος τε αὐτὸ τῷτο τένομα ἐκάλεν ὧδ' ἦν ὡς τινα ἕτερον ἐπέλεγον δὲ κιθαρωδικός. Τῶτων δὲ διαλέλαγμένων καὶ ἄλλων τινῶν ἐκ ἕξιν ἄλλω εἰς ἄλλο κατὰ χρῆσθαι μέλῃς εἶδος. —Μετὰ δὲ ταῦτα παρ' ἑαυτῶν τῶ χρέον, ἀρχοντες μὲν τ' ἀμύσου παρὰ νομίας

This passage, with what has been said, being sufficient to give a general idea of the scope of melopœia, I shall pass to that of harmonic.

We have already seen, that the theoretic division of music was by the author, whose distribution I have followed, subdivided into natural and artificial, and that harmonic came under the latter. The ancients, indeed, seem not all to have treated the science under such narrow limits, the physical properties of sound, the ratios of intervals, &c. appearing to have been considered therein, by the Pythagorean and other schools, as well as the structure of the systems in use. But not to stop at this objection, which is not very material, thus far must be allowed, that harmonic, as to the greatest part of its scope, was an artificial science, its most considerable object being to explain and teach the denominations, positions, powers, &c. of the sounds of the system, and all other inventions in use, for facilitating the study of music, or bringing it to greater perfection. Now, in this short view of the science, which is sufficient for our purpose, we may already begin to account for the difference between its doctrines and those of melopœia, artificial sciences being but too prone to admit doctrines for convenience, that are repugnant to the truth. This was indeed the case with harmonic, as the two doctrines of the tones sufficiently testify; the origin of which I shall now consider.

νομίας ποιηταὶ ἐγένοντο, φύσει μὲν ποιητικοὶ, ἀγνώμονες δὲ τοῖς τὸ δίκαιον τῆς μύσης καὶ τὸ νόμιμον. βακχέοντες, καὶ μᾶλλον οὐκ ἀκούοντες κατεχόμενοι ὑφ' ἡδονῆς, κερσαίνοντες δὲ θρήνησαν ὕμνοις καὶ παιῶνας διδυμεύουσιν· καὶ αὐλωδίας δὲ τῇ κιθαρωδίᾳ μιμούμενοι, καὶ πάντας οὖς πάντα ξυνάγουσιν. Plat. de Legibus, lib. iii.

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The musical doctrine I look upon as the most antient, and that which exhibited the modes in their proper character; for which reason, it could not avoid making a part, and probably the most essential one, of the doctrines of melopœia. The precise time when it first came into use, I pretend not, in the midst of uncertainties, to be able to determine; but we may, perhaps, be near the truth, if we refer its origin to the age of Pythagoras, if the eighth string of the lyre was really, as we are told, the addition of that philosopher. The story of his discovering, at a forge, the ratios of hypate, mese, paramese, and nete, is well known. These ratios, which gave rise to the harmonic proportion, and the numbers of which were so vainly applied afterwards, by philosophers, to all the parts of the universe, were drawn from the Dorian species of diapason, which we cannot suppose to have been well understood, till its form existed upon the instrument. What doctrine of the modes prevailed, before the addition of the eighth string, we can only guess at; for it was a question, even with Aristotle (19), how the heptachord lyre had been adjusted; and Nichomachus speaks of that instrument so

(19) Διὰ τί οἱ ἀρχαῖοι ἐπὶ τετραχόρδῳ ἀρμονίας ποιῶντες ἢ ὑπάτην, ἀλλ' ἢ τὴν νήτην κατέλιπον; πότερον τὸ τοῦ Πυθαγόρου, ἀμφοτέρως γὰρ κατέλιπον, ἢ δὲ τείτην ἐξήρην; ἢ ὅτι, ἀλλ' ὅτι ἡ βαρυτέρα ἰσχύει τὴν ὀξύτεραν φθόγῳ, ὥστε μᾶλλον ἢ ὑπάτη ἀπεδίδα τὸ ἀκρίφωνον ἢ ἢ νήτην ἐπὶ τὸ ὀξύ δυνάμεως μᾶλλον, τὸ δὲ βαρὺ εἶον φθιγγεῖσθαι. Aristot. Probl. sect. 19. qu. 7.

Διὰ τί οἱ ἀρχαῖοι ἐπὶ τετραχόρδῳ ποιῶντες τὰς ἀρμονίας ἢ ὑπάτην ἀλλ' ἢ τὴν νήτην κατέλιπον; ἢ ὅτι τὴν ὑπάτην, ἀλλὰ τὴν νῦν καλεσμένην ᾠχρῶς ἀφῆραν καὶ τὸ τονιάζον διάστημα, ἐχρῶντο δὲ τῇ ἐσχάτῃ μέσῃ τῇ ἐπὶ τὸ ὀξύ πυκνῇ, διὸ καὶ μέσῃ αὐτὴν προσεχόρευσαν; ἢ ὅτι ἦν τῇ μὲν ἀνω τετραχόρδῳ τελευτῇ, τῇ δὲ κάτω ἀρχῇ, καὶ μέσον εἶχε λόγον τῶν ἄνω ἀκρων. Probl. sect. 19. qu. 48.

confusedly,

confusedly, as to have given room for a supposition (20), that it had received two forms, the seven strings answering, in the oldest form, to our notes *e, f, g, a, b^b, c, d*, and in the new one, to *e, f, g, a, b, d, e*. However this may have been, neither the musical nor the harmonic doctrines could be then in use, exactly as they were taught in after times; and the probability seems to be, that the modes were in those days characterized by the species of the lesser consonances, diatessaron and diapente; but the theory of the Dorian species of diapason, we may be sure, took place about the time, when the lyre was brought to that compass; and the other species, though they might have existed before, in the melody of particular instruments; as for instance, the barbarous Phrygian, upon its flute, could hardly have been taken into the Greek theories of the science, till the extension of their own favourite instrument had brought the diapason under consideration: so that the origin of the musical doctrine of the modes, is, with great probability, to be referred to this improvement of the lyre.

I come now to the harmonic doctrine, for the origin of which we must look to the invention of the system. The greater perfect system, upon whose pitch the modes depended, by the harmonic doctrine, consisted, as I have already shewn, of fifteen sounds, answerable to those of the lyre; and it is reasonable

(20) Ut ex his difficultatibus nos expediamus, duas, non opiniones, sed ætates statuere debemus, quibus aliter obtinuerint intervalla in septem chordis. Meibomius in Nichomachum, p. 52. See also Nichomachus, p. 9. v. 14. & seq. & p. 17. v. 24. & seq.

to think, that the structure of it was not settled, till the instrument had been again extended to the compass of a disdiapason, by the additions of the tetrachords hyperbolæon and hypaton, and of the sound proslambanomenos: for this change the age of Alexander the Great may, perhaps, be a probable æra; for, in the musical problems of Aristotle, I observe no mention of the new tetrachords, though many of the questions concern the strings of the lyre; and yet there is no doubt of their being in use in the time of Aristoxenus, his disciple. Should I be near the truth in this, the musical doctrine will then appear to have been earlier than the harmonic, by the whole period from Pythagoras to Alexander. But, without being solicitous about the precise time when the harmonic doctrine was introduced, I shall, with more certainty, endeavour to point out what must have given occasion to it.

The study of the music of the antients, though they seem not to have much used composition in parts, must yet have been very perplexing, from the variety only of the tones and genera; and some help might well be thought necessary, even in the time of the octachord lyre. But when seven new strings were given to the instrument, and these placed not all at one end, but three at the acute, and four at the grave, the eight old strings, upon which the characteristic species of melody for each mode had been always exhibited, became confounded by these additions; and possessing now the center of the lyre, it was difficult to distinguish them, and to preserve for each mode its proper character as before. This difficulty must also have been increased, by the change
introduced

introduced in the practice of the science; for both the players and composers, having now a greater latitude, would not fail to take advantage of the enlargement; and though the skilful among them might, in their excursions upon the new strings, preserve a due attention to the proper characters of the several modes, yet the melody peculiar to each could not be so easily comprehended, when carried beyond its usual limits. An artificial help, therefore, to the learner, was now become indispensably necessary; and with this view, I make no doubt, but that the system was invented. It was, indeed, admirably well contrived for the purpose; for its succession being the same with that of the lyre, in its Dorian tuning, with which the Greeks must have been the best acquainted, as being the proper mode of their favourite instrument, it was the most easy to be learnt and retained, and the knowledge of this succession was all now required; for, by imagining only the system removed to some other pitch, and tuning all the strings in the same relations to a new mese, the melody of the old eight strings was thereby changed, and a new species of diapason gained thereon, without the trouble of studying the the musical doctrine, which was now left to the musician, or melopoëius, who was answerable for the choice of the successions he selected for his various subjects, whilst the harmonician followed him through the intricacies of his compositions, by the easier method of the transposition of the system. And this I apprehend to have been the origin, both of the system itself, and of the harmonic doctrine of the tones. But now, as it often happens, what was designed for the improvement of the science, became,
in

in time, the ruin of it; for, after the reception of the diſdiapafon ſyſtem, the elementary treatiſes, as we ſee by what is left to us of them, became filled with this new doctrine of the parts and relations of the ſyſtem, of its tranſpoſitions, of the poſitions of the meſe, of ſounds in power, and ſound by poſition, and many other doctrines flowing from the ſame ſource, till, in time, the ſyſtem itſelf came to be conſidered as the true type of a mode; and a number of new modes were introduced, that were grounded on no other principle than this, which I ſcruple not to call a falſe doctrine of them, though the antients admitted it for convenience, as I have ſhewn, and thereby almoſt loſt their impreſſions of the new one.

Having thus pointed out the origin of theſe two doctrines, as far as was poſſible, from a general view of them, and without entering into a critical examination of the many paſſages of the antients, that might help us to greater certainty, I ſhall now proceed,

Fifthly, To ſhew how far the preceding explanations may be ſupported by arguments, or warranted by the teſtimony of antient writers.

Here I muſt repeat, that the harmonic doctrine of the tones, as I have explained it, is found, expreſſly delivered under its proper head, in almoſt every writer on the ſubject; and that the doctrine of the ſpecies of diapafon is found alſo in the harmonic treatiſes, though not under the head of tones, nor expreſſly applied to them. Now, this being the caſe, I ſhall have no occaſion to waſte time in bringing

particular proofs, to support the explanations I have given of these two doctrines separately. What immediately lies upon me is, to prove, that the doctrine of the species of diapason had a connection with the modes; and to justify the manner, in which I have combined the two doctrines in the canon, and in the diagrams framed from it. With this view, therefore, I shall direct my arguments to the proving of the five following points.

1. That the doctrine of the species of diapason was a doctrine of the modes.

2. That this doctrine was not distinct from, nor independent of, the harmonic, but so connected with it, that a mutation, according to the one, produced the alteration required by the other.

3. That the species of diapason, as taken practically on the lyre, lay all at the same pitch.

4. That this pitch was the diapason, between the strings hypate meson and nete diezeugmenon, as they stand in the diagram.

5. I shall endeavour to shew, that it was the musical doctrine, and not the harmonic, that exhibited the true character of the modes, as considered in respect to their effects.

To begin with the first point. That the doctrine of the species of diapason was a doctrine of the modes, might, perhaps, be inferred from some of the definitions given of a mode; but as these definitions are mostly intermixed with, if not drawn solely from, the harmonic doctrine, and hence have given room for disputes, I shall rest this point upon three arguments, in which the definitions are not concerned.

First,

First, Because the species are denominated after seven of the modes, which could hardly have arisen from any other cause, than their connection with them.

2dly, Because the whole reasoning of Ptolemey, in the sixth, seventh, eighth, ninth, tenth, and eleventh chapters, of his second book, tends to reduce the modes to the number of the species of the diapason; which, as far as his opinion goes, is an express confirmation, that the musical doctrine was a doctrine of the modes.

3dly, Because Ptolemey is not single in this opinion, as some may have thought, but is supported in it by the Aristoxenian writers, who, though they admitted thirteen modes, upon the false principle of the harmonic doctrine, did, in fact, reduce them to eight, on the very principle of Ptolemey, calling the Hyperionian mode, which gave the same species with the Mixolydian, the acuter Mixolydian; and calling also the Æolian, the graver Lydian; the Ionian, the graver Phrygian; the Hypoæolian, the graver Hypolydian; and the Hypoionian, the graver Hypophrygian; the former of all which modes gave, respectively, the same species with the latter. So that, to make their doctrine answer to that of Ptolemey, there wanted but the reduction of one mode more, which was their Hyperphrygian. And it is not to be conceived, but that they saw this mode also to be a repetition of the Hypodorian; but as this repetition was at the distance of an octave, and the other five were repeated at the difference only of a semitone in pitch, they could not have called this the acuter Hypodorian, without using the same expression in too great a latitude

tude of sense. And this might be the reason why they omitted taking notice of that circumstance attending this mode; though we might perhaps, with safety to the argument, go a step farther, and suppose, that the Aristoxenians might think a difference of an octave in pitch worthy of a distinct denomination, though they would not allow it to that of a semitone. And this is the more probable, as we find they did not absolutely reject these modes, as Ptolemy did, but admitted them upon the principles of the harmonic doctrine, though, by their denominating five of them from the genuine modes, it is plain they acknowledged them to be superfluous, in respect to the musical. I cannot say more to this point, without defrauding the subsequent ones; and I shall therefore leave it, with this remark, that whatever is proved of the subsequent points, and especially of the fifth, must hold true of this, *à fortiori*; so that it may really be said to have the support of the whole testimony.

2. To the second point, the first diagram speaks very strongly; for had the two doctrines no relation to each other, no such coincidence, as is there found, could have been expected, in the result of their combination; and this proof, from the coincidence of the doctrines in the diagram, becomes much stronger, if we attend to these two circumstances. First, That in the canon, upon which the diagram has been constructed, there is no strain of either doctrine, but a plain and natural combination of them, as they are stated separately by the antient writers. And secondly, That the species of diapason, as brought out by this combination, all fall upon the strings of the old octachord

octachord lyre, where it was natural to expect them; for when the lyre had but eight strings, the species could be taken no where else; and it cannot be thought, but that, after the extension of the instrument, they were taken at the same pitch as before, unless we should suppose, that the new strings brought with them an immediate change, both of the theory and practice of the science, which is most improbable. We see our modern harpsichords have more keys given to them than those of the last century; and yet neither the positions of the cliffs, nor any other circumstances attending our theories of the science, have been altered by them. But that this point may not rest wholly on the circumstances of the diagram, I shall produce four passages, that plainly shew the relation between the two doctrines. The first from Aristoxenus.

“Now, as some of those [systems] which we employ in music, are simple, and others mutable, we must treat of mutation; and first of this, what mutation is, and how it is accomplished; now, I say, there happens, as it were, a pathos in the melody; afterwards, how many mutations there are in all, and according to what intervals (21).”

In this passage, the author is speaking of the sixth division of harmonic, which was mutation; and more particularly of mutation with respect to tone, which, in this division, was always treated upon the

(21) Ἐπεὶ δὲ τῶν μελοδυσμένων ἐστὶ τὰ μὲν ἀπλά, τὰ δὲ ἀμετάβολα, [corrigé, ἐμμετάβολα] περὶ μεταβολῆς ἀν' ἑνὶ λεκτέον· πρῶτον μὲν αὐτῶν, τί ποτ' ἐστὶν ἡ μεταβολή, καὶ πῶς γινόμενον. Λέγω δ' οὗτος πᾶσι τινὲς συμβαίνοισιν ἐν τῇ δ' μελωδίᾳ τάξει. Ἐπειτα πόσαι εἰσὶν αἱ πόσαι μεταβολαί, καὶ κατὰ πόσα διαστήματα. Aristox. p. 38. v. 7.

principle of the harmonic doctrine, and is so here; but he tells us, there was a pathos in the melody, which can only relate to the musical doctrine, and therefore shews their connection. In the next passage, from Aristides Quintilianus, it is more plainly hinted at.

“ For if a certain type of the voice follows each system, it is manifest, that the species of the melos will be altered with the harmonics (22).”

But the two following passages from Ptolemey will put the matter out of dispute.

“ For we are not to imagine this constitution of the mutation, according to the tone, established for sake of graver or acuter voices, (since the intension, or again the remission of whole instruments, suffices for such a difference, no alteration being produced in respect of the melos, the whole being executed by performers of graver or acuter voices); but with this view, that the same melos, begun by the same voice, now from acuter places, and now from graver, may produce a certain change of ethos; because that in the permutations of the tones, the extremities of the voice do not answer to both the extremities of the melos; but in one, the extremity of the voice always falls short of that of the melos; and in the other, the extremity of the melos of that of the voice: so as that the same melos, which at first (meaning in the Dorian) answered to the compass of the voice, now falling short

(22) Ἐν ἑκάστῳ συστήματι καὶ ποίῳ τις ἐπακολουθεῖ τῇ φωνῇ τύπος δῆλον ὡς ἅμα τῇ ἀρμονίᾳ καὶ τὸ τῷ μέλῳ εἶδος ἀλλοιωθήσεται. Aristid. Quintil. p. 24. v. ult.

of it, and now again exceeding it, may give to the ear the impression of another mode (23)."

"For of the mutations in respect to tone, so called, there are two principal differences, the one by which we run through the whole melos in an acuter tension, or again in a graver, preserving always the succession of the species. The other, by which there is an alteration, not of the whole melos in its tension, but only of a part thereof, from the succession it set out with. Wherefore, this last might be called rather a mutation of the melos, than of the tone; for, according to the former, the melos is not altered, but the tone (tension) of the whole; whereas, according to the latter, the melos varies from its proper order, and the tension varies not as tension, but as on account of the melos. Hence the former does not strike the sense with an idea of that alteration, according to power, by which the ethos is changed, but only of an alteration in respect to the being acuter or graver; whereas the latter causes the sense to drop from the usual and expected melos, the succession being preserved some time, and then making a tran-

(23) Ὅουδὲ γὰρ ἕνεκεν τῆ βαρυτέρων ἢ ὀξύτερων φωνῶν ἕννεμιον ἂν τὴν σύστασιν τὴ κατὰ τὸν τόνον μεταβολῆς γεγεννημένην· ὅποτε πρὸς τὴν τοιαύτην διαφορὰν ἢ τὸ ὄργανον ὅλων ἐπίτασις, ἢ πάλιν ἀνεσις ἀπαρκεῖ, μηδεμιᾶς γε ὡς ἀλλαγῆς πρὸς τὸ μέλον ἀποβλεπόμενης, ὅταν ὅλον ὁμοίως ὑπὸ τῆ βαρυφωνοτέρων ἢ τῆ ὀξυφωνοτέρων ἀγωνιστῶν διαφανήσῃ· ἀλλ' ἕνεκα τῆ κατὰ τὴν μίαν φωνὴν τὸ αὐτὸ μέλον ποτὲ μὲν ὑπὸ τῆ ὀξύτερων τόπων ἀρχόμενον, ποτὲ δὲ ὑπὸ τῆ βαρυτέρων, τροπὴν τινα τῆ ἡδὺς ἀποβλεπὴν τῇ μηκέτι πρὸς ἐκότερα τὰ πέρατα τῆ μέλους συναπαρτιζέσθαι τὰ τῆ φωνῆς ἐν τῇ τῶν τόνων ἐναλλαγῇ· ἀλλ' αἰὲν προκαταλήγειν, ἐπὶ μὲν ὀξύτερα, τὸ τῆ φωνῆς πέρατα τῆ τῆ μέλους· ἐπὶ ἢ τὰ ἐναντία, τὸ τῆ μέλους πέρατα τῆ τῆ φωνῆς· ὥστε τὸ ἐξ ἀρχῆς ἐφαρμόσαν τῇ διασάσει τῆ φωνῆς μέλος, πᾶν μὲν ἀπολείπον ἐν τῇ μεταβολαῖς, πᾶν ἢ ἐπιλαμβάνον, ἐτέρῃ ἡδὺς φωνασίαν παρέχει τῇ ἀκοαίς. Ptol. Harm. lib. ii. c. 7.

sition.

sition to another species, whether according to genus or tension (24)."

3. The third point will receive much support from these two considerations; first, That diapasons, variously divided and variously bounded, also seem to form a more complicated theory, than can well be supposed to have prevailed in early times, when music had not yet lost its natural simplicity; and secondly, That the compass of the instrument, in its octachord state, would not admit of any variation, in respect to the bounding sounds. But there is testimony to this point also: I shall cite one pretty remarkable passage, from Aristides Quintilianus, which will be sufficient, as the proof of the next point necessarily carries this along with it. Aristides, who, in his explanations of the harmonic doctrines, follows the method of the Aristoxenians, coming under the head of systems, to treat of the species of diapason, after explaining their situation in the immutable system, and assigning their denominations, immediately subjoins:

"Hence it is plain, that if we make our first sign the same, though denominated in different cases from

(24) Εἰσὶ δὲ καὶ παρὰ τὸ ἔγω λεγόμενον τόνον, μεταβολῶν δύο πρῶται διαφοραί· μία μὲν κατὰ τὸ ὅλον τὸ μέλ^{ος} ὁξύτερα τάσει διέξιμην, ἢ πάλιν βαρυτέρα, τηρέντες τὸ διὰ παντὸν τῷ εἶδους ἀκόλουθον· δευτέρα δὲ, κατὰ τὸν ἔχ^{ον} ὅλον τὸ μέλ^{ος} ἐξαλλάσσει τῇ τάσει, μέρος δέ τι παρὰ τὴν ἐξ αρχῆς ἀκολουθίαν· διὸ καὶ καλοῦντ' αὐτὴν αὐλήν, τῷ μέλει μάλλον, ἢ τῷ τόνῳ μεταβολή. Κατ' ἐκείνην μὲν γὰρ ἐκ ἀλλάσσει τὸ μέλ^{ος}, ἀλλ' ὁ δῖος κατὰ ταύτην δὲ, τὸ μὲν μέλ^{ος} ἐκτρέφει τὴν οἰκίας τάξεως· ἢ δὲ τῷ αἰσθησὶ φαντασίαν ἐτερότητ^{ος} κατὰ τὴν δύναμιν, ὅφ' ἡς κινεῖται τὸ ἦθος, ἀλλὰ μόνως τῆς κατὰ τὸ ὁξύτερον ἢ βαρυτέρον· αὐτὴ δὲ ὥσπερ ἐκπέπλην αὐτὴν ποιεῖ τῷ συνήθους καὶ προσδωκομένου μέλει, ὅταν ἐπὶ πλέον μὲν συνέρηται τὸ ἀκόλουθον, μεταβαίνει δὲ τῷ πρὸς ἕτερον εἶδος, ἢτοι κατὰ τὸ γέν^{ος}, ἢ κατὰ τὴν τάσιν. Ptol. Harm. lib. ii. cap. 6.

a different

a different power of sound, the quality of the harmony will become manifest, from the succession of the sounds in order (25).

In this passage, it is to be observed, that the term *σημειον*, a sign, signifies a musical character, such as the antients used to write over the words of a song, to express the air, or tune; and that each of these characters represented not a sound of the system, subject to transposition, but, like the notes of our gamut, a sound of a certain pitch, or at least liable to very small variations only, from the generic differences. The term, therefore, is properly used here by Aristides, in opposition to *φθόγγον*, a sound of the system, whose pitch depended on the mode in which it was employed. As to the expression, quality of the harmony, it evidently respects the species of diapason; so that the author plainly means to tell us, that if we begin a diapason always with the same note, the succession of its sounds, that is, the manner in which it is divided, will determine the species: and so far he speaks to our purpose. But this point will receive farther proof, from what we shall say upon the next.

4. The fourth point being the natural result of the combination of the two doctrines, carries a sort of conviction with it; and the stronger, as not only the seven modes, but the whole fifteen, assist the proof, as will appear, on examining the table I have given of them: for there the sounds lying between the

(25) *Ἡ δὲ δὴ τέτα φανερὴν, ὡς καὶ ταυτὸν ὑποθεμένοις σημειῶν πρῶτον, ἀλλήλῃ· ἀλλήλῃ δυνάμει φθόγγη καλονομαζόμενον, ἐκ τῆς τῇ ἐφεξῆς φθόγγῃ ἀκολουθίας τὴν τῆς ἀρμονίας ποιότητά φανεράν γινεσθαι συμβαίνει.* Arist. Quint p. 18. v. 7.

strings hypate meson and nete diezeugmenon fall out so, as to give the proper species for every mode ; which circumstance will not attend any other boundaries, common or varying, without altering the positions of the mese, and thereby disturbing the harmonic doctrine. But we have from Ptolemey sufficient testimony to this point. In the eleventh chapter of his second book, of which I have given a translation above, we see, that the diapason in question is chosen by him, to exhibit the divisions, that belong to the several species. 'Tis true, he seems to insinuate, that any other diapason might have been chosen, were it not for the difficulty the voice finds in running to the extremes : and in this, as a theorist, he is right ; for musical relations and proportions, considered as objects of mere speculation, may be conceived at any pitch ; but in the case before us, it was the practice of the instrument, that governed the theory. The pitch of the lyre had been made to answer to the double octave of the human voice ; the pitch of the system, in the Dorian mode, answered to that of the lyre ; and, in every other mode, the difference in pitch from that of the Dorian was determined. Now, under such limitations, that took their rise from practical music, it is evident, that no other diapason, but that instanced by Ptolemey, could have served his purpose.

But that the boundary of the species is rightly adjusted in the diagram, there is yet another strong testimony, from the tables of Ptolemey, annexed to the fifteenth chapter of book second. To make this proof evident, it will be necessary to give some account of this part of his work.

In the twelfth chapter of book second, he begins to treat of the sections of the harmonic canon, for proving, by experiment, the truth of the ratios he had delivered for the genera, and proposes to exhibit these sections for each genus, in all the seven tones. Then, after shewing the defects of the monochord canon in use, and (chapter thirteenth) the insufficiency of the improvements made thereon by Didymus, he proposes, and explains, his own method of applying the canon to eight strings tuned in unison. But, before he proceeds to give the sections upon all the tones, he thinks it necessary to shew, by experiment, the advantage of his own numbers, above those of preceding harmonicians; for which purpose, the exposition of them in one tone, that is, in the immutable system, was sufficient. Accordingly, in chapter fourteenth, (the greatest part of which has been lost, and is supplied from conjecture by Wallis) and in the tables annexed, he gives the sections of the Dorian species of diapason; where the diazeuctic tone, lying in the middle, left a complete tetrachord on each side, for the exposition of the generic numbers; and compares his own ratios with those of Archytas, Eratosthenes, Aristoxenus, and Didymus. Then, in the fifteenth chapter, he proceeds, as he proposed, to give the sections according to his own ratios, for the genera most in use in all the seven tones: these sections he has included in two sets of tables, each set containing seven, *viz.* one for each mode; and each table being again subdivided into five, for the generic differences. Now, of these two sets of tables, the first justifies the diagram; for here he has given his numbers upon the eight strings, from nete diezeug-

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menon to hypate meson; so that the diagram and the tables agree in every respect, except the generic differences, which I had no occasion to consider. In the second set, indeed, the numbers are applied to the diapason, from mese to proslambanomenos; and there being no explanation in this chapter of the distinct use of this second set, nor any reason assigned for giving it, but the convenience of having the option, to begin the tuning from nete or mese (26), this may seem to furnish an objection to the proof proposed to be gained from these tables: but if we look forward to the second chapter of his third book, we shall there see his meaning; which I shall proceed to explain.

Having, in the sixteenth chapter, shewn how the numbers, given in the two sets of tables, are to be applied to an octachord canon, he proposes, in the first chapter of the third book, to shew, how the divisions for the fifteen sounds of the whole disdiapason system might, if required, be found, according to the same ratios he had already assigned for the extent of a diapason. Now, for the doing this, two inconveniences were to be obviated. First, If the instrument, on which this experiment was to be tried, was to receive an addition of seven strings, to complete the disdiapason, and these additional strings were all to be in unison with the eight before applied, it would happen, that, in shifting the moveable magas, or bridge, to the sections required, those strings, which were to exhibit the acute sections of the tetra-

(26) "Ἐν ἡχοῦν αὐτὸ ὁποτέρως ἀντὶ τῶν ἀρχῶν περαιώμεθα ποιῆσαι τὰς ἀρμονίας. Ptol. Harm. p. 174. v. ult.

chord hyperbolæon, would have so little space left between the magas and the extremity of the string, that it would be difficult for them to yield a distinct sound. And secondly, The canon must be crowded with these additional divisions, which would also have its inconvenience. To remedy this, he proposes several methods; the first of which is, to fit the instrument with fifteen strings, of which the eight, that were to receive the divisions from mese to nete hyperbolæon, should be in unison, at the pitch of mese, and the other seven in unison, at the pitch of proslambanomenos. Now, as the succession from the string proslambanomenos to the string mese, though it varied with the modes, was yet, in the same mode, always the same with that from mese to nete hyperbolæon, the difference of a diapason in pitch excepted, it is plain, that, under this adjustment, a canon divided for the one diapason would serve equally for the other, the difference in pitch being established before in the open strings. By this method, therefore, the divisions for the acuter diapason would be as large as those for the graver, and the canon needed not to be crowded with a greater number of divisions, than had been used for the single diapason, by reason of its double application. The other methods, which he proposed, I need not go through the explanation of, farther than to remark, that, for those, as well as for this, a fresh set of numbers was necessary, those contrived for the succession of the diapason, from hypate meson to nete diezeugmenon, being no-ways applicable to a succession, which began from proslambanomenos or mese. And this, it seems, was his reason for giving the second set of tables in his fifteenth chapter,

chapter, as appears from his own words, in the second chapter of the third book.

In general, for such uses, as comprehend a diapason only, it behoves us to employ, out of the numbers exhibited, those which contain the section from nete diezeugmenon, that the melos may be taken in the middle tensions; but for such as comprehend the disdiapason, those exhibited from nete hyperbolæon, or mese, that the tuning may be adjusted at both extremities alike (27).

Here we see he fully explains his former meaning; and, in recommending his first set of tables for the uses of a diapason, gives the same reason for the choice of that diapason in particular, as he assigns for it in the eleventh chapter of the second book, *viz.* that the melos may possess the middle of the instrument. But it must be observed, that when the whole disdiapason has been adjusted by the second set of tables, the tunings of the eight strings, from nete diezeugmenon to hypate meson, will come out, for every mode and genus, the very same, as if they had been tuned by the first set: so that no inference can possibly be drawn from thence, either that the author has varied as to his own meaning, or that the diagram, which I have given, does not correspond with it.

(27) Καθόλου ὁ προσεκτεον, τὰς ἐκκειμένους διειδμὰς τῇ μὲν τὸ διαπάσων μόνον περιχέουσας χρήσεις, τὰς ἀπὸ τῆς ὑψίτης τῇ διεzeugμένην ἔχοντας τὴν καθόλου μὲν ἐν τῇ μεσότητι τάσει ἐκτελεσθῆναι τὸ μέλῳ, τῇ δὲ τὸ δις διαπάσων, τὰς ἀπὸ τῆς ὑψίτης τῇ ὑπερβολαίαν, ἢ τῆς μεσῆς, ἐκτελεσθῆναι: ἵνα κατ' ἀμφοτέρων τῶν ἀκρῶν ἢ ὁμοίων ἐφαρμοζέσθαι δύνηται.
Ptol. Harm. p. 228. v. 15.

5. I come now to the fifth point, which has been the subject of much dispute, and which might well afford an endless controversy, whilst the union of the two doctrines was not understood; for whatever reasons, from probability or testimony, might be urged in favour of the musical, the harmonic doctrine appearing to be still better supported, and seeming to contradict the other, was sure to suspend the decision, at least, if not gain one in its own favour. But the agreement between the two doctrines being shewn, and their views distinguished, the testimony borne to the harmonic doctrine will no longer be in the way of the musical, and we may safely allow whatever appears favourable to the latter, its full weight. I shall therefore offer, in support of this point, the six following arguments.

First, No one accustomed to hear and judge of the effects of music, can conceive, that a mere alteration in pitch of the same melody, though in the hands of the most artful master, can have so powerful an effect, to change the mode or style of composition, as an alteration of the melodious succession: or, if this should be asserted, it might be disproved, by an appeal to the works of all the composers of eminence among the moderns; who having, in all the variety of their modulations, but one change, that truly answers to the musical doctrine of the modes, *viz.* the change of the succession of the same key, from the major third to the minor, have all reserved this change for their pathetic passages. I shall give but one instance of it, from our countryman Purcell, who, in his *Mad Bess*, has so happily introduced this change, upon the words, “ Cold and hungry am I
“ grown,”

“grown,” that if the performer does justice to the composition, the hearer can scarce help fancying himself affected with the very sensations, with which the starts of frenzy are there pictured.

2dly, If I have been right in assigning a greater antiquity to the musical doctrine, than to the harmonic, the effects of the modes must, in earlier times at least, be referred to the former. And this carries with it an argument for later times also; for the old principle of the modes could not be suddenly changed. And though it may be urged, from the introduction of the eight spurious modes, that the harmonic doctrine came, in time, to be considered as a principle independent of the other, this will only prove a corruption of the better doctrine, which may safely be admitted; though that the musical doctrine was ever quite disused, unless in very late times indeed, I much doubt, as some traces of it are found in almost all the writers come down to us.

3dly, Ptolemy's rejecting the eight modes, that wanted the support of the musical doctrine, is another proof. For, if the harmonic doctrine had been the more essential of the two, those modes ought to have been preserved. Nor does the admission of six of them, by the Aristoxenians in earlier times, weaken the force of this argument: for though the rejecting them is conclusive, against the harmonic doctrine, as far as any weight is allowed to the opinion of the rejecter, the argument from their admission will not conclude in its favour, till it be proved, that in the use the species of diapason afforded by these modes was not attended to, but only the order from the proslambanomenos, in power of each, as the favourers of
the

the harmonic doctrine seem to suppose: and this proof it would not be easy to obtain, since each of these modes had its species belonging to it, and was only exceptionable, because it repeated the melody of one or other of the seven.

4thly, The denominations given by the Aristoxenians to five of the spurious modes, after the genuine ones, whose species they afforded, furnish another strong proof; for this could scarce have happened, if the pitch of the system had been the principal distinction.

5thly, We find in Plutarch, Pliny, and other authors, the invention of particular modes ascribed to particular musicians; which may be accounted for, on the supposition, that the modes were so many different species of diapason; since it requires great art and skill to introduce agreeably melodies, to which the ear has not been accustomed: but the taking the same melody at a different pitch is a variety, for which the inventor would hardly have had his name so carefully delivered to posterity.

But 6thly, there are passages, that strongly confirm the preceding arguments. Aristoxenus speaks of modes of the *melopœi* (28), by which I understand him to mean the species of diapason, in contradistinction to the modes of the harmonicians; for I suspect the modes of *melopœia*, mentioned by Aristides Quintilianus (29), and divided by him into three kinds, *viz.* *nomic*, which was *netoides*; *dithyrambic*, which was *mesoides*; and *tragic*, which

(28) Pag. 40. v. 21.

(29) Pag. 29. v. ult.

was hypatoides; and which are also mentioned by other writers, of a low age (30); these modes, I say, I suspect to be founded on distinctions not in use so early. The use of the species of diapason in melopœia is confirmed also by Bacchius, who, immediately after explaining them, expresses himself thus:

“ So many, therefore, in the art of music, are the species of the consonances, by which every melopœia is formed (31).”

And also by Aristides Quintilianus, who concludes his doctrine of the species, with these words:

“ And thus much suffices concerning systems, which the antients also styled the ethic principles (32).”

For, by systems, we are here to understand systems in the general sense, as defined and treated by the Aristoxenians, whom Aristides follows in this part of his work, and more particularly the three consonant systems, whose species he had just been describing.

The passage I have already cited from Plato, serves also strongly to support this point; for the terms, species, and figure, are there used in the strict technic sense, and not merely to express variety, as appears from his speaking, in the same passage, afterwards of the species of melos, the sense of which cannot be mistaken; and that he is speaking of music, in

(30) Martianus Capella, p. 189. v. 21. Manuel Bryennius in Oper. Wallisii, p. 503. v. 14.

(31) Τοσῶτα μὲν ἔστι τῶν συμφωνιῶν τὰ εἶδη ἐν τῇ μεσσηνίᾳ τέχνῃ, δι' ἃν πᾶσα μελοποιία συνίσταται. Bacch. Sen. Introd. Art. Music. p. 19. v. 18.

(32) Περὶ μὲν ἐν συστημάτων, ἃ καὶ ἀρχαῖς δι' παλαιὰ τῶν ἰσθμίων ἑκάστην, ἀρκεῖται τελεῖται. Aristid. Quintil. p. 18. v. 12.

respect to its effects, is manifest, from the whole passage: so that, though the modes are not directly mentioned, they are necessarily to be implied. But Ptolemey is still more express; for, in the passage cited above, from book second, chapter seventh, he directly affirms, that the *ethos*, or character of the mode, depended on the musical doctrine, and not on the harmonic; and is still more explicit, to the same purpose, in the passage cited also from the sixth chapter of the same book. These two passages are so clear, that there is no evading their testimony, but by supposing the author to be singular in his opinion; which there is, indeed, room to think was his case, with respect to many of the musicians, his contemporaries; for the pains he has taken to clear up and distinguish the two doctrines, is a sufficient proof, that mistakes had prevailed concerning them; but that what he has advanced is not repugnant to the doctrines held in earlier antiquity, has, I think, been amply shewn. I shall now close these proofs with three remarkable passages from Plutarch's dialogue on music, which will all become intelligible from the explanation given of this subject, at the same time, that they will serve to confirm it. The first respects the invention of the Mixolydian mode, which we shall see he treats as a species of diapason, telling us between what sounds of the system it lay, and in what part of the diapason the diazeuctic tone was situated; in both which circumstances, the passage agrees with the preceding explanation, and with the first diagram.

“ Lyfis informs us, that Lamprocles the Athenian, seeing, that it (the Mixolydian harmony) had not the

disjunction (diazeugtic tone), where almost every one imagined (meaning in the middle of the diapason, for so it was in the Dorian, which was most familiar to the Greeks), but at the acute, made the figure of it such as that from paramese to hypate meson (33)."

The next passage informs us of a very remarkable circumstance, *viz.* that, after the addition made to the lyre of the tetrachord hypaton, no use was made of those strings in the Dorian mode, though they were employed in the rest. Speaking of the earlier times, and arguing, that the antients had omitted many things, rather from choice than ignorance;

"It is manifest (says he), that, in respect to the [tetrachord] hypaton, it was not through ignorance, that they abstained, in their Dorian [compositions], from this tetrachord; for they employed it in the rest of the tones, as plainly knowing it; but, for the sake of preserving the ethos, they left it out in the Dorian tone, as respecting the beauty thereof (34)."

From this passage it is evident, that the Dorian melody, which, in its proper character, according to the musical doctrine, was terminated by hypate meson, was in such esteem with the Greeks, from a long habit of hearing their best pieces composed in that succession, and within that compass,

(33) Λύσις ἣ (σησι) Λαμπρόκλεα ἢ Ἀθηναῖον συνιδόντα ὅτι καὶ ἐν ταῦτα ἔχει ἢ διάζευξιν, ὅπερ αὐτὸν ἀπαίρει ἀπὸ τοῦ ξυ, καὶ τὸν αὐτοῦ ἀφ' ἑαυτοῦ τὸ γῆμα ὅς τὸ δὲ πρὸς ἀμείσσει ἐπὶ ὑπάτῃ ὑπάτων. Plutarch. Dial. de Musica.

(34) Διὸν ἣ καὶ τὴν περὶ ὑπάτων, ὅτι καὶ δι' ἀγνοίαν ἀπέχοντο ἐν πῶς δαρίοις τὰ τετραχόρδον τέττα, αὐτίκα ἐπὶ τῇ λοιπῇ τόνῳ ἐχρῶντο, δηλοῦντι ἐιδότες διὰ ἣ καὶ τὰ ἡδύς φυλακὴν ἀφ' ἑαυτοῦ ἐπὶ τῇ δαρίῳ τόνῳ, τιμῶντες τὸ καλὸν αὐτῶν. Ibidem.

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that the additional strings could not be ventured upon in that mode, though in the rest they were admitted.

The last passage is that, which I have mentioned above to have been censured by Meibomius: we shall see here, that the author is expressly arguing for confining harmonic within its proper sphere, and not suffering it to encroach on melopœia, by engaging with the rules of propriety, in the use of the elements; so that it both justifies and explains the distinction I have made above, between the two sciences, and turns the censure of Meibomius upon himself, who seems, indeed, without excuse, unless we suppose him to mean only, that Plutarch, in giving the Aristoxenian division, ought not to have sunk any part of it: which remark would have been just, if Plutarch had given the division as such; but he does not name the school, though he adopts their distribution of the science as far as the six divisions he approved; so that no inconsistency can be charged upon him. The latter part of this passage also confirms the distinction I have made between the two doctrines, and their use in the two sciences; so that it assists most of the preceding arguments and proofs: and here I shall therefore rest this point, which will scarce be thought to need farther support.

For it is evident, that the immediate objects of harmonic are genera, *viz.* those of the harmonies, intervals, systems, sounds, tones, and systematic mutations; and farther than this it cannot go: so that we ought not to expect it should determine how far the poet, in his music, has properly assumed the

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Hypodorian tone, for instance, for the outset; or the Mixolydian and Dorian, for the conclusion; or the Hypophrygian and Phrygian, for the middle; since the harmonic treatise does not extend so far, but has need of many others. For of the force of propriety it cannot judge, since neither the chromatic nor the enharmonic genus will ever contain a perfect force of propriety, such as to discover the ethos of the composition, that being the business of the artist. And again, it is plain, that the voice [pitch] of the system is to be distinguished from the melopœia wrought in the system, the consideration whereof does not belong to the harmonic treatise (35). Having now finished my head of proofs, I come,

Lastly, To consider how far this subject has been understood by Meibomius, Wallis, and some few others that have wrote since; and in what respect their explanations differ from my own.

In respect to the opinion of Meibomius, Malcolm tells us; that that writer, in his notes on Aristides Quintilianus, affirms the differences of the modes,

(35) Διὸλον γὰρ ὅτι ἡ ἁρμονικὴ γυνὴν τε τὴν τῆς ἡρμωμένης καὶ διασημα-
των καὶ συσηματων καὶ φθόγων καὶ τόπων καὶ μεταβολῶν συσηματικῶν ἐστὶ
γνώσκειν, πρῶτον δὲ ἐκείνη ταύτην προσελθεῖν οἰήσῃ. ὥστε καὶ ζητεῖν
ὡς ταύτης τὸ διαγιῶναι δύνασθαι πότερον οἰκείως εἰλησεν ὁ ποιητής,
ὁμοιον εἰπεῖν, ἐν μέσαις τὴν ὑποδάειον τόνον ἐπὶ τὴν ἀρχὴν, ἢ τὴν μιζολυ-
δίων τε καὶ δάειον ἐπὶ τὴν ἑκάστην, ἢ τὴν ὑπερφύγιον τε καὶ φρύγιον ἐπὶ τὴν
μέσσην. Οὐ γὰρ διατείνει ἡ ἁρμονικὴ πραγματεία πρὸς τὰ πιαῦτα,
προσδίδται δὲ πολλῶν ἐτέρων. τὴν γὰρ τὴν οἰκίωσιν δυνάμιν ἀγνοῖ.
Οὐτε γὰρ τὸ χρωματικὸν γένος, ὅτε τὸ ἐναρμόνιον ἡξί ποτὲ ἔχον τὴν
οἰκίωσιν δυνάμιν τελείαν καὶ καθ' ἣν τὸ τῆς πεποιημένης μέλους ἡδὲ
ἐμφανίζεται, ἀλλὰ τὸτο τῆς τεχνίτης ἔργον. Φανερόν δὲ ὅτι ἐτίει τῆς
συσηματικῆς ἡ γυνὴ τὴν ἐν τῇ συσηματικῇ καὶ ἀσκεισασθῆναις μελοποιίας, περὶ
ἧς ἐκ ἐστὶ διαόρησαι τὴν ἁρμονικὴν πραγματείαν. Plutarch. Dial. de
Musica.

upon which all the different effects depended, to have been only in the tension, or acuteness and gravity of the whole system (36). And indeed, in the note on page 13. verse 4. of that author (37), which, I presume, Malcolm had under his eye, Meibomius clearly decides for the harmonic doctrine, as he does also in his note on page 2. verse 1. of Euclid's Harmonic Introduction (38). But in this last note, he had just before told us, that tone was by the antients also called harmonia, and species of diapason (39). And, in the conclusion of his note on page 1. verse 10. of the same author, he delivers himself more fully to the same purpose.

“ The antients (says he) having considered several species of diapason in the bisdiapason, called these also harmonics. Whence we read Dorian, Phrygian harmony, in the best writers. The same were again called tones and tropes, or modes (40).”

In

(36) Malcolm's Treatise on Music, p. 540.

(37) Hic autem locus oppidò notandus est contra recentiorum de tonorum effectibus opiniones, illorum enim varietates, *so*, *la*, acuminis atque gravitatis differentia veteres unanimi consensu definiunt—Acumen autem ac gravitatem nihil varietatis cantilenæ adferre contra omnem eruditam vetustatem, imo communem sensum existimant. Meibom. in Arist. Quint. p. 219.

(38) Tonus seu modus est tonus systematis harmonici, hoc est bisdiapason aut simpliciter diapason differentia; ut Phrygius tonus à Dorio nulla alia re differt, quam quod totum Phrygii systema acutius sit toto Dorii systemate, tono, qui est in ratione super-octava. Meibom. in Euclid. Introd. Harm. p. 46.

(39) Toni vocabulum quatuor modis accipitur;—hic idem est quod modus; cum dicimus tonus, sive modus Dorian, Lydian, Mixolydian; veteribus quoque harmonia adpellatur, et species diapason. Ibidem.

(40) Cum autem plures diapason species in bisdiapason antiqui spectarent, illas quoque adpellabant harmonias. Unde Dorian, Phrygia

In his note also on the passage I have cited above, from Aristides, page 18. verse 11. he explains the expression, quality of the harmony, to signify the species of diapason, or tone (41), which is the sense I have put upon it. We see therefore, that though he hastily ascribes to the harmonic doctrine alone the effects, which I have supposed to arise only from the musical, yet he clearly admits both the doctrines to be warranted from antiquity; and I am glad to have so far the support of this learned critic's opinion: but of the connection between the two doctrines, as I have explained it, I see no trace in his notes; nor is it to be imagined, but that, if he had seen it, he would have enlarged upon it.

In respect to Dr. Wallis, though he had the advantage of the notes of Meibomius, who had cleared up so many difficulties, and had also taken under his own management the text of Ptolemy, the author, of all others, the most likely to have given him a thorough insight into this subject, yet we find him not only defective in his explanations of it, but, contrary to his usual accuracy, even in misleading his readers by false doctrines. With respect to the musical doctrine, if we may judge by his silence, he appears to have seen less of it than Meibomius; for, in the appendix to his edition of Ptolemy, wherein he

gia harmonia, apud optimos autores legimus. Deinde eadem toni et tropi, seu modi sunt adpellati. Meibom. in Euclid. Introduct. Harm. p. 42.

(41) Id est quæ sit species octachordi, seu quis tonus; nam duobus modis usurpatur vox *ἁρμονία*, uno pro genere enarmonio, altero pro tono, seu tropo, seu modo; quæ significatio est huic loco propria. Meibom. in Arist. Quint. p. 230.

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undertakes to explain the harmonic elements, after treating of the species of diapason, he gives not the least hint, that they had any relation to the modes, except by giving their denominations; and in doing this, he expresses himself (42) as if he thought these denominations rather assigned for some separate reason, than on account of their connection with the modes; though, as he was going immediately to the doctrine of the tones, this was the place, where he might have been expected to have taken notice of the connection between the two doctrines, if he had observed it. In treating of the modes, indeed, though he explains them on the foot of the harmonic doctrine only (43), he assigns some of his author's reasons for reducing them to seven (44). And his note also on the beginning

(42) Atque hæc quidem diapason species septem sua singulæ fortiebantur nomina; prima dicta est Mixolydia; secunda, Lydia; tertia, Phrygia; quarta, Doria; quinta, Hypolydia; sexta, Hypophrygia; septima, tum Locrensis tum Hypodoria. Tandem de modis, seu tonis dicendum restat. App. ad Ptol. p. 311.

(43) Modus itaque, seu tonus, prout hic sumitur, denotat vocis locum, non quo una vox, sed quo tota vocum series, seu systema canitur; acutiorem puta, gravioremve. Utpote prout apud nos *mi* canitur, nunc in *b f b mi*, nunc in *elami*, nunc in *alamire*, &c.: Sic apud illos verbi gratia, paramese potestate (quod tantumdem est atque nostrum *mi*), posita erat nunc in paramese positione, nunc in positione nete diezeugmenon, nunc in mese, &c. Ibid. p. 312.

(44) Contra hos, qui tonos, seu modos, sic augent per hemitonia disputat Ptolemæus, cap. 7, 8, 9, 10, 11. lib. ii. docetque modorum variorum usum non in hunc solum finem introductum, ut acutior gravioreve fit totius cantus tenor; quippe huic sufficeret cantoris vox acutior gravioreve; aut musici organi ad hosce tenores accommodatio.—Sed eo potissimum fine fuisse introductum, ut in ipsius cantus curriculo transitus fiat de modo in modum, quam vo-

ginning of chapter eleventh, book second (45), rightly explains the species of diapason, as they lay between hypate meson and nete diezeugmenon: but this interpretation is formally introduced with a *nempe hoc vult*, as if he thought his author singular in this doctrine, and looked upon the doctrine itself, as stated in that passage, rather as explanatory of an incidental circumstance attending the harmonic doctrine, than meant to assign the true doctrine of the modes. This is manifest, from his drawing the same inference, both in this note and elsewhere (46), from this musical doctrine of the species, as he had drawn before from the harmonic one, viz. that the tones thus

cant (*μεταβολὴ κατὰ τόνον*) mutationem secundum tonum: quod à nostris fit mutata clavis signatura adhibitis mollis et duri notis, &c. — Et propterea tonos distinctos ponendos esse docet omnino septem, Mixolydium, Lydium, Phrygium, Dorium, Hypolydium, Hypophrygium, et Hypodorium. Totidemque admittit hodierna musica pro varia clavis signatura. Ibid. p. 313.

(45) Nempe hoc vult: diapason illud expositum ab hypate meson ad nete diezeugmenon; hoc est (in musica hec sermo), ab elami ad elami, his vocibus post primam cani in tono Mixolydio, *fa, sol, la, fa, sol, la, mi*; quæ est prima species diapason, habens *mi*, seu tonum diezeugmenon in loco præcedente, seu acutissimo: his in Lydio, *sol, la, fa, sol, la, mi, fa*; quæ est species secunda diapason, habens *mi*, in loco secundo ab acutissimo: in Phrygio, *la, fa, sol, la, mi, fa, sol*: in Dorio, *fa, sol, la, mi, fa, sol, la*: in Hypolydio, *sol, la, mi, fa, sol, la, fa*: in Hypophrygio, *la, mi, fa, sol, la, fa, sol*: in Hypodorio, *mi, fa, sol, la, fa, sol, la*; quæ sunt tertia, quarta, quinta, sexta, et septima species diapason; habentes *mi*, in loco tertio, quarto, quinto, sexto, et septimo, ab acutissimo: quæ omnia in musica nostra mollis et duri notis varie positis indicantur, ut mox dicemus. Ibid. ad Gal. p. 136.

(46) Ut soli supersint Ptolemæi septem, Hypodorium, Hypophrygius, &c. totidemque non scit hodierna musica posito *mi* in F, G, A, b, c, d, e. Ibid. p. 154, ad finem.

settled

settled by Ptolemey are acknowledged by the moderns, in their seven positions of the *mi*. For nothing favours less of the musical doctrine, than what the moderns hold in this respect. And it is manifest also, from his neglecting to assist the explanation of this doctrine of Ptolemey, by any citations from other authors, that speak to the same purpose; which he would not have failed to do, if it had struck him as an antient and genuine theory of the modes. But it is plain to me, that, however he may have penetrated the sense of these particular passages, he did not grasp their consequences. This the mistakes he has fallen into sufficiently shew; which I shall now proceed to point out, and which a thorough comprehension of the subject could not but have prevented.

In speaking of the mese of the Hypophrygian mode, he explains it by *c*#, instead of *f* # (47), a mistake, which, I think, must be imputed to his haste.

In the table of the modes, which he has given from Meibomius (48), he has placed the letters in use with the moderns, against the Hypodorian, as well as the Dorian, making *A* answer to the *proslambanomenos* of the former, as well as the latter; by which, it is to be presumed, he means them to express the absolute pitch of the sounds in the Dorian, and in the Hypodorian, to denote only the general relations of the system; but this should have been farther explained.

(47) Not. ad Ptol. p. 154. lin. 41.

(48) P. 155.

But his greatest mistake, and which I know not how to reconcile to his usual caution, is in the method he gives for finding the places of the *meses* for the seven modes. Not only is this method erroneous, but he gives it expressly as the method of Ptolemy (49), though no such direction is to be found in his work. As I have already given Ptolemy's method; in our modern terms, for the sake of brevity, from his tenth chapter, where it is delivered, I shall here give that of Wallis, in the same terms, that the difference between the two may more easily be seen.

“ First pitch the Dorian, which is the middle tone, suppose in A; rise a fourth to D, for the Mixolydian; fall a fifth to G, for the Hypolydian; rise a fourth to C, for the Lydian. Then begin from the Dorian again, and fall a fourth to E, for the Hypodorian; rise a fifth to B, for the Phrygian; and fall a fourth to F#, for the Hypophrygian (50).”

By this method, we see the *meses* of the Lydian and Hypolydian are brought out at *c* and *f* natural; whereas, by Ptolemy's, they come out at *c*# and *f*#, where I have placed them.

This mistake, I apprehend, the doctor was led into by the eleventh chapter of Ptolemy's second book, where the *meses* of the Lydian and Hypolydian are settled in *trite diezeugmenon* and *lichanos meson*; which strings, in their natural situation, in the Dorian mode, were tuned to *c* and *g* natural;

(49) Hanc autem methodum adhibet Ptolemæus in tonis suis septem designandis, &c. — primum omnium facit, &c. secundo tonum sumit, &c. App. ad Ptol. p. 313 & 314.

(50) Ibid. p. 313. lin. ult. usque ad p. 315. lin. 20.

but,

but, in this chapter, the author means only to fix the numerical string allotted for each mese, without regard to its pitch, which was to be regulated by the distances assigned for the tones in the tenth chapter. And by these distances, and all other concurring circumstances, it is manifest, that the two strings in question were, in these modes, to be made a semitone, more acute than their natural situation, as I have already observed, in explaining the harmonic doctrine. How, therefore, the doctor could so far wander from the true theory, which lay before him in the text of his author's tenth chapter, as to substitute a different method, and deliver it, with great prolixity of explanation, as the method of his author, is what I know not how to account for, much less reconcile to his accustomed care and fidelity. What is still more extraordinary, is, that, after finishing the directions and explanations of his own method, he, in the very next paragraph, cites the very tenth chapter of Ptolemey, where the method is given (51), in order to infer with his author, that the Mixolydian tone was distant from the Lydian a hemitone; the Lydian from the Phrygian, a tone, &c. though this inference, which is true only from Ptolemey's method, directly contradicts all he had been delivering. Could he overlook the falseness of this inference, whilst he was taking so much pains

(51) His positis, inde colligitur (ut cap. 10.) toni Mixolydii à Lydio distantiam limma, seu crassius loquendo hemitonium; hujus à Phrygio, tonum; hujus à Dorio, tonum; Dorii ab Hypolydio limma; Hypolydii ab Hypophrygio, tonum; hujusque ab Hypodorio, itidem tonum. App. p. 315. lin. 21.

with

with the subject? But not to insist farther on the slips of a writer, to whom the learned world stands so highly indebted, I shall take leave of him, with this remark only, that whatever he may have seen of the truth of these doctrines himself, his explanations have not succeeded in making the subject clear to succeeding writers; those I have seen having either adopted the harmonic doctrine only, or been so confounded between the two, as to give a right account of neither. This has, in particular, been the case with Malcolm, who, in his Treatise on Music, explains that of the antients, and has taken some pains to reconcile the two doctrines of the modes. The greatest part of what this writer delivers is not only false, in respect to the order, positions, and, indeed, almost every other circumstance attending the modes, but, at the same time, scarce intelligible; or, if any meaning can be put upon it, it is too foreign to the truth of either of the doctrines, to be worth considering. I shall content myself with citing a passage from him, where his reasoning is the clearest, and where we may see, that, after all the pains he has taken to reconcile the two doctrines, he owns himself unable to make any sure decision upon the subject.

“ He (Ptolemy) says, in the beginning of that chapter (cap. 7. lib. ii.), the mutations, which are made by whole systems (which we properly call tones, because these differences consist in tension), are infinite, with respect to possibility, as sounds are; but actually, and with respect to sense, they are finite. All this seems plainly to put the difference of the tones only in the acuteness or gravity of the whole; and, how do their differences consist in tension, which signifies

signifies a certain tenor or degree of tune? and how can they be called infinite, if they depend on the constitution of the octave? Yet, elsewhere, he argues, that they are no other than the species of octaves, and as such, makes their number seven; and accordingly, in all his schemes, sets down their different modulations. But, in chapter sixth, he seems more plainly to take in both these differences; for he says, there are two principal differences with respect to the change of the tone, one, whereby the whole song is sung higher or lower, the other wherein there is a change of the melody to another species than it was begun in; but this, he thinks, is rather a change of the song, or melos, than of the tone; as if again he would have us think, this depended only on the acuteness and gravity of the whole. So obscurely has the best of all the antient writers delivered himself on this article, that deserved to have been most clearly handled. But, that I may have done with it, I shall only say, it must be taken in one or other of the senses mentioned, if not in both; for another, I think, cannot be found (52).”

Notwithstanding the confusion in this author's explanations, and his leaving the question thus undecided, it is his account of the modes, that seems chiefly to have been adopted by compilers since. And hence we find nothing better in the *Cyclopædia* of Chambers, or the *Musical Dictionary* of Cressineau, than a repetition of these doubts and perplexities. Amongst the French writers, Brossard, in his *Dictionnaire de Musique*, throws no light upon this sub-

(52) Malcolm's *Treatise on Music*, p. 538. lin. ult.

ject, his account of the modes respecting chiefly those of much later times, which were distinguished into plagal and authentic, and with which the present question has very little concern. Nor have I met with any thing satisfactory from their academicians, though some of them appear to have taken great pains with the subject. The writer amongst these, who seems the most conversant with the music of the antients, is Monsieur Burette, who, in the *Memoirs of literature*, has furnished many pieces on the various branches of the science. Amongst others, there is a dissertation of his on the *melopœia* of the antient music, which the learned academician should rather have intitled a dissertation on harmonic; for it contains an explanation of the elements of that science. This author seems little apprized of the musical doctrine; nor has he given a right account even of the harmonic one, having followed Wallis in his mistake, in respect to the method, by which Ptolemey fixed the positions of his seven modes (53), and even drawn the same contradictory conclusion (54):

(53) Qu'ainsi on devoit renfermer dans l'espace d'une octave tous les modes, dont le Dorien devoit occuper, comme le centre, les six autres etant disposez de façon, que le Mixolydien fut d'une quarte plus haut que le Dorien; l'Hypolydien d'une quinte plus bas que le Mixolydien; le Lydien d'une quarte plus haut que l'Hypolidien; l'Hypodorien d'une quarte plus bas que le Dorien; le Phrygien d'une quinte plus haut que l'Hypodorien; et l'Hypophrygien d'une quarte plus bas que le Phrygien. *Dissertation sur le Melopée, &c.*

(54) D'ou il paroît, qu'à compter de l'Hypodorien, qui est le mode le plus bas, il y a de celui ci à l'Hypophrygien l'intervalle d'un ton; de l'Hypophrygien à l'Hypolydien un autre ton; de l'Hypolydien au Dorien, un demiton; de ce dernier au Phrygien, un ton; du Phrygien au Lydien encore un ton; et du Lydien au Mixolydien, un demiton. *Ibidem.*

whence

whence it appears, that he relied on the doctor's appendix for this part at least of his explanations. In his notes on the dialogue of Plutarch, printed also in the same memoirs, the not understanding the musical doctrine has led him into a very false explanation of the passage cited above, concerning the invention of the Mixolydian mode. He sets out (55) with excluding the only supposition, that could lead him to the sense of his author, by denying, that the octachord system could have any thing to do with this invention; and then flies (56) to two other suppositions, *viz.* that either the hendecachord or disdiapason was here meant, both which are equally wide of the truth. The public is much indebted to this writer, for the laborious collections he has made in these notes, concerning the antient musicians; but his harmonic explanations are not always to be relied on.

(55) Lamprocle n'est point ici donné comme l'auteur de l'harmonie Mixolydienne, il en est regardé seulement comme le reformateur. Mais en quoi pouvoit consister cette reforme? C'étoit, comme le dit Plutarque, à déterminer le véritable système de cette harmonie, ou de ce mode, quant à sa disjonction, ou à l'arrangement des divers tetrachordes, qui composoient ce système. En le réduisant à l'étendue de l'octave, ou de l'octachorde, c'est à dire du double tetrachorde disjoint; le lieu de cette disjonction est unique, et par conséquent n'est point equivoque, comme on le peut voir par la progression de ce système, que voici, *mi, fa, sol, la, si, ut, re, mi*; il ne s'agit donc point ici du double tetrachorde disjoint. Note 114.

(56) Mais l'hendecachorde, ou le triple tetrachorde disjoint, pouvoit être le système, dont parle icy Plutarque—supposé qu'il soit icy question du système complet de l'ancienne musique, &c. Ibidem.

The last writer, that I have seen, who has treated this subject, and with whose opinion I shall conclude these sheets, is the anonymous author of a letter to Mr. Avifon, concerning the music of the antients, published with the essay of the latter on musical expression, in 1753. This author, who professes but a slender knowledge of the theory, either of modern or antient music, has, in a few pages, discovered himself to be possessed of more than his modesty will permit him to lay claim to. In the matter of the tones, however, (such is the fatality, that seems to have constantly attended this doctrine) he does not appear to have succeeded better than the writers that have gone before him. He blames Sanadon and Cerceau, for affirming, in their observations on Horace, carm. 5. 9. that the Dorian mode answered exactly to our A-mi-la with a minor third, and the Phrygian to our A-mi-la with a major third. Now, that these French critics, though right, inasmuch as they seem to consider the modes as different species of diapason, are, nevertheless, mistaken in their application of them, does, indeed, appear from my first diagram, where it may be immediately seen, that what they assert of the Dorian and Phrygian modes, is true only of the Hypodorian and Lydian; and had the anonymous writer gone no farther in his censure of them, I should readily have joined with him: but he rejects the musical doctrine entirely, and admits only of the harmonic. " Surely (says he (57), speaking of what these critics advance) this is a musical error, and a dream from the ivory gate. Two modes

with the same tonic note, the one neither acuter nor graver than the other, make no part of the old system of modes." And, agreeably to this opinion, he had before (58) described the seven modes, as reducible to one mode, taken higher or lower; which, he says, may be called the mode of A, and must have had a minor third, as *c* natural is a minor third to A-mi-la. Whether this be a just account of the nature of the modes, I must now submit, having said already what has occurred to me, in support of the contrary opinion.

To conclude, if what has been said should be thought to establish any certainty concerning this matter, I should hope it might tend to revive an inquiry into the music of the antients, which seems to have suffered an interruption from the difficulty of arriving at a right understanding of this material branch of it. Such an inquiry may, perhaps, be judged well worth pursuing, not only from the advantages, which modern music might derive from it, but also for the sake of improving the art of poetry, and of better understanding and tasting the noble remains we have of the antient poets; the greatest beauties of whose works, especially the lyric and dramatic, are probably lost to us, for want of seeing their connection with this science.

LXIX. *An Enquiry into the Measure of the Roman Foot ; by Matthew Raper, Esq; F. R. S.*

Read May 15, 22,
and June 5, 1760.

THE methods that have hitherto been taken to discover the measure of the Roman foot, will, upon examination, be found so unsatisfactory, that it is no wonder the learned are not yet agreed in that point.

The antient foot-rules now remaining ; the representations of the foot in sculpture ; and the measure of it, derived from the congius, differ so much among themselves, and from each other, as to be insufficient evidences separately : and the great disagreement of the foot from the congius, with the rest, has not hitherto been satisfactorily accounted for.

The foot-rules found in old ruins at Rome, are of various lengths ; and the age of none of them being certainly known, no precise measure can be determined from them, otherwise than by taking a mean from such as appear to be most perfect. But though this may have been the foot in use at some time or other, yet as these rules are probably of different ages, both the greatest and the least of them may have answered to the standards of their times.

For though we have no account of any alteration ever made in the standard of the Roman foot, yet the wear of a standard measure by use, and the making new to replace the old ones, must, in all probability, create a difference ; especially, as the Romans had not those inducements to so precise an accuracy

accuracy in these matters, as the later discoveries in natural philosophy (particularly the invention of the pendulum) have introduced among the moderns. Add to this, that the different state of Europe, which has, for some ages, been divided into many considerable kingdoms and sovereign states, independent on each other, equally civilized, and carrying on a more constant and regular mercantile commerce with each other, than was known to the Romans, must necessarily introduce more frequent enquiries into the weights and measures of the different states, and a more careful examination of the respective standards of each, than the Romans could have any occasion for: and use in these matters, is the parent of accuracy.

We can arrive at no greater certainty from the marbles, than from the foot-rules. These, indeed, do not differ so widely from each other, as the rules; which seems to be the reason why most of the writers on this subject have given them the preference: but of the four that are extant, no two agree in the same measure; nor is the age of any one of them known: and as they were intended for representations only, and not for use, their accuracy may reasonably be doubted.

Festus, Frontinus, and Rhemnius Fannius, say, the side of the quadrantal (which contained 8 congii) was a Roman foot (1). A standard congius of
Vespasian

(1) Quadrantal vocabant antiqui quam ex Græco ἀμφορέν dicunt, quod vas pedis quadrati octo et quadraginta capit sextarios. Festus in v. Quadrantal. Pes quadratus concavus capit amphora trimodia

Vespasian is still in being, and has been measured by several learned men; but the foot derived from it exceeds those on the marbles, and the greatest number of the foot-rules, so much, that Mr. Greaves could find no better way of accounting for so great a difference, than by supposing what Festus and Fannius say (for he does not quote Frontinus), to be a vulgar error (2): whereas the name of this standard shews its figure to have been a cube (3); which adds probability to their testimony, that its side was intended for the measure of the foot.

The measures of public roads in the itineraries, can be of little use in this enquiry; for they omit fractions, and we do not know whether the distances of the towns are reckoned from the market-places or from the gates; but a difference of half a mile in sixty, is equivalent to the tenth part of an inch in the foot: therefore, no exact measure is to be expected from thence, even though the modern mensurations of Cassini, Riccioli, and others, were more unexceptionable than they really are.

trimodia [read, amphoram trimodiam]. Frontinus, in Expositione Formarum. Though the passage is corrupt, there can be no doubt of the meaning.

Pes longo spatio latoque notetur in anglo,
 Angulus ut par sit quem claudit linea triplex;
 Quattuor ex quadris medium cingatur inane,
 Amphora fit cubus: quem ne violare liceret,
 Sacravere Jovi, Tarpeio in monte, Quirites.

Rh. Fannius, de Pond. et Mens.

(2) See Dr. Birch's edition of Greaves's Works, p. 228.

(3) Quæ illi κύβος, nos quadrantalia dicimus. A. Gellius, l. i. c. 20.

The

The distances between the antient mile-stones are not liable to these objections; and if a sufficient number of such as stand nearest to Rome were carefully measured, their authority would be considerable. But I do not find that any are now standing within thirty miles of that city, nor that any of these have been measured, or even any in Italy; and provincial measures are not of equal authority.

There is still another method, whereby we may discover the measure of the Roman foot; which is, from the remains of the antient buildings now standing at Rome: and though many have made use of some single parts of them for this purpose, yet no one hath hitherto compared the measures of the principal parts of any one of them with each other, which is the only way to discover the measure whereby a building was constructed.

With this view, therefore, I shall carefully examine the measures of the buildings contained in that inestimable treasury of antient Roman architecture, intituled, *Les Edifices antiques de Rome*, and published at Paris, by Mons. Desgodetz, in the year 1682.

In order to this, it will first be necessary to ascertain the proportion of the Paris foot (the measure used by this author) to some known English standard.

The Paris foot is one sixth part of the toise in the Chatelet; which was renewed in the year 1668 (4), and the new standard has continued in use ever since.

(4) See De la Hire's Dissertation on the Roman Foot, in the Memoirs of the Academy of Sciences for the year 1714; where he gives an account of this renewal of the standard in the Chatelet.

In the year 1742 (5), the Royal Academy of Sciences at Paris, at the request of the Royal Society of London, sent over a measure of half the toise of the Chatelet; from which Mr. Graham determined the proportion of the Paris foot to that of London, to be as $1065,41\frac{2}{3}$ to 1000. Mons. Le Monnier, of the Royal Academy of Sciences, from the same originals, found their proportion as 864 to 811, or as 1065,351 to 1000. The difference is inconsiderable, and we may, without injustice to Mr. Graham's known skill and accuracy in these matters, suppose their true proportion to be as 1065,4 to 1000.

Mr. Graham's measure of the London yard, together with that of half the toise of the Chatelet, are deposited in the archives of the Royal Society at London, and of the Royal Academy of Sciences at Paris; and whenever I shall mention the London foot, without specifying any particular standard of it, I would be understood to mean this measure.

In this enquiry, we are to seek a common measure to the several parts of each building, that shall not differ very widely from some assumed magnitude of the Roman foot: and though we might take this assumption from any of the antient foot-rules now remaining, yet the nearer it is taken to the truth, the better guide it will be to us, and the more it will facilitate our enquiry. Now, as a mean measure, derived from these rules, will, probably, be nearer the truth, than either the greatest or the least of them, so one that shall include such other remains of antiquity, as have hitherto been made of to discover

(5) See Philos. Trans. N^o 465.

the measure of the Roman foot, will be still more unexceptionable, as the writers on this subject are not agreed, which of the different authorities is to be preferred.

The representations of this foot in sculpture, are four in number; one on the sepulchral monument of Cossutius, formerly in the Colotian gardens at Rome; another on that of Statilius, in the Belvedere; a third on that of M. Ebutius, in the Villa Mattei (6); and the fourth on a marble, without inscription, dug up of late years in the Via Aurelia, which, being in the possession of the Marquis Capponi, is called, by P. Revillas, the Capponian foot.

Most of the early writers on this subject have expressed their measure of the Roman foot by a diagram; and Snellius (7) observing, that the paper contracted in drying, after the impression was taken off, endeavoured to make a proper allowance for it. But Greaves, finding the measures of these figures to differ in different copies of the same impression, took another method; and seems to have been the first that compared the original figures on the monuments of Cossutius and Statilius with a modern standard. This he did with such care and diligence, that his measures deserve a particular examination.

The London foot, which he used upon this occasion, was taken from the iron standard of 3 feet in

(6) Picard and Auzout (in a paper called *De Mensuris*, hereafter quoted) have given the measure of the foot on this monument of Ebutius for the Colotian foot.

(7) See his *Dissertation on the Roman Foot*, in the 3d vol. of *Saggi di Dissertazioni Accademiche di Cortona*.

the Guild-hall, London (8); which having been long since lost, or destroyed, we have nothing left whereby to discover its true magnitude, but the measures others have taken of it, and those which have since been taken of such magnitudes as Greaves had compared with his copy of it.

Snellius, from a measure sent him of this iron standard (9), determined the proportion of the Rhynland to the London foot, as 1000 to 968. The Rhynland foot, according to Picard (1), contains 696 such parts as the Paris foot contains 720: whence the proportion of the latter to this measure from the iron standard, is as 1065,4 to 997 nearly. Eifenschmid found the Rhynland foot to contain 1391,3 such parts as the Paris foot contains 1440 (2); which gives 1065,4 to less than $996\frac{1}{2}$, for the proportion of the Paris foot to that of the iron standard. Huyghens makes the Paris to the Rhynland foot as 144 to 139 (3); whence the proportion of the former to Snellius's London foot, will be nearly as 1065,4 to $995\frac{1}{2}$. But there is reason to believe, that Huyghens's measure of the Rhynland foot was too small (4).

By

(8) Greaves, p. 223.

(9) Eratosthenes Batavus, p. 125. 141.

(1) See a paper intituled *De Mensuris*, in *Divers Ouvrages de Mathematique et de Physique*, par Messrs. de l'Academie Royale des Sciences, Paris 1693, in folio, and afterwards printed in the 4th vol. of *Ouvrages adoptez*.

(2) Eifenschmid de Pond. et Mens. vett. p. 94.

(3) Horolog. Oscill. part iv. prop. 25.

(4) Picard, in his *Voyage d'Uranibourg*, p. 64. edit. Amst. says, "Passant par Holland je pris l'occasion de verifier la proportion

By these comparisons it appears, that Snellius's measure of the London foot, from this iron standard, was at least 3 parts in 1000 shorter than Graham's London foot.

Our countryman Norwood, in the year 1635, measured the distance between London and York, in order to determine the length of a degree on the meridian; which he found to contain 367196 London feet of this iron standard (5). The French found the measure of a degree, in the latitude $66^{\circ} 20'$, to be 57438 toises, and at the equator 56783. Hence the measure of a degree in $52^{\circ} 44'$ (the middle latitude between London and York) will be found to be 57276 toises, or 343656 Paris feet. These numbers give the proportion of the Paris foot to that of the iron standard, as 1065,4 to 997,1 —, wanting somewhat less than 3 parts in 1000 of Graham's London foot.

Picard's paper *De Mensuris*, quoted above, and another on the same subject by Auzout, printed with it, contain some measures, which Greaves had before compared with his London foot. Both these papers were written after the renewal of the standard of the Chatelet, in 1668. The former is so full of inaccuracies and mistakes, that little use can be made of it; but Auzout's measures appear to be accurate; and as he seems to have taken his Paris foot from the

portion du pied de Paris à celui du Rhin, dont l'original est à Leyde; laquelle proportion me parut estre exactement comme 720 à 696, au lieu de 720 à 695, que j'avois supposée dans la mesure de la terre." This latter is Huyghens's proportion.

(5) See Norwood's *Seaman's Practice*.

toise in the Chatelet for this purpose, it was probably a correct measure of that standard.

Such of his measures as answer to Greaves's, are here reduced to thousandth parts of the London foot, reckoning his Paris foot to contain 1065,4 such parts.

	Greaves.	Auzout.	Diff.	Diff. per foot.
The Statilian foot	972.	969,96	— 2,04	2,1
The braccio of Florence	1913.	1908,84	— 4,16	2,18
The braccio of Siena	1974.	1973,21	— 0,79	0,4
Pætus's palm	732.	731,35	— 0,65	0,89

All these differences fall the same way, and shew, that Greaves's London foot bore a less proportion to Auzout's Paris foot, than that of 1000 to 1065,4. The Statilian foot, and the braccio of Florence, give the greatest differences; one is carved in relievo, and the other is a rod; so that both are subject to diminution, by time and use. The palm is engraved; and Auzout, from the deviation of Pætus's Greek and Roman foot, from the common received proportion of 25 to 24, suspected, that the latter was lengthened by wear; if so, the like must have happened to the palm. How their measures of the braccio of Siena come to differ so little, I cannot account for. But we can draw no certain conclusion from this comparison, till we know whether any, and what, allowance is to be made for the diminution or increase of these magnitudes, in the interval between Greaves's and Auzout's measuring them.

For this purpose, I shall compare Auzout's measures, with those taken by P. Revillas, with a correct

rect London foot (6), between fifty and sixty years after. These he has converted into Paris measure, by the proportion of 864 to 811; and I have reduced them back again, according to the same proportion.

	Auzout.	Revillas.	Diff.	Diff. per foot.
The Statilian foot	969,96	969,79	— 0,17	0,18 —
The Ebutian foot	972,91	972,22	— 0,69	0,71 —
Pætus's palm	731,35	732,65	+ 1,3	1,78
Pætus's foot	966,26	968,74	+ 2,48	2,57
	or 967 —		or 1,75	1,81

Hence the carvings appear to have suffered very little diminution in this interval; but Pætus's measures, which are engraved, and in constant use as public standards, have been considerably lengthened. Therefore, allowing for both these circumstances, we may conclude, that Greaves's London foot wanted 2 parts in 1000, to be to Auzout's Paris foot, in the proportion of 1000 to 1065,4.

Greaves's measure of the door-case of the Pantheon, 9 inches within the jambs, is 19,602 of his London feet and decimal parts (7). Desgodetz found the measure of the same on the inside 18 feet $4\frac{7}{8}$ inches Paris measure, and on the outside 18 feet $4\frac{1}{3}$ inches, their difference is $\frac{1}{2}\frac{3}{4}$ of a Paris inch; the depth of the jambs is 4 feet $10\frac{1}{4}$ inches Paris measure; and

(6) *Esattissimo piede Inglese*. See *Saggi di Differtazioni Accademiche di Cortona*, vol. iii. p. 113. I have been credibly informed, that this London foot was given him by the late Martin Folkes, Esq; and that it was made by Siffon, according to Graham's measure.

(7) Greaves, p. 348.

9 London inches are equal to 8,447 Paris inches and decimals. Hence the measure of this door-case 9 London inches within the jambs, should, according to Desgodetz, be 18,3676 Paris feet and decimals. But 19,602 is to this number, as 1065,4 to 998,3; and Greaves's London foot wanted $1\frac{7}{10}$ to be to Desgodetz's Paris foot, in the proportion of 1000 to 1065,4.

Greaves says, most of the white marble stones on the pavement of the Pantheon contained exactly 3 of those Roman feet on Cossutius's monument (8). Their Paris measure, according to Desgodetz, is 2 feet $8\frac{1}{2}$ inches, which is equal to 2885,46 London parts. This number divided by 3, gives but 961,82 such parts; whereas Greaves's measure of the Cossutian foot was 967 parts of his London foot; which must therefore have wanted above 5 parts in 1000 of the proportion before-mentioned. But it is probable they measured different ranges of these stones.

The measure of the Paris foot, which Greaves received from Mons. Hardy, was taken from the old standard in the Chatelet (9), and contained 1068 such parts as his London foot contained 1000. These numbers are in the proportion of 1065,4 to 997,57; therefore, if the new standard did not differ from this old one (and no such difference appears to have been intended), Greaves's London foot must have been 2,43 parts in 1000 shorter than Graham's.

(8) Greaves, p. 211.

(9) See Mons. Hardy's Letter, in Greaves's Works, p. 447. The measure he sent was only half the Paris foot; but he tells Greaves, it was taken from the standard, under his own inspection, with great care.

All these comparisons shew Greaves's measure of the London foot to have been shorter than Graham's. But they are not all of equal authority. The measures of Snellius depend upon the correctness of three several feet; that is to say, his copy of the iron standard, the Rhynland, and the Paris foot; whereas the rest (except Norwood's) depend only on the Paris foot, applied to the same magnitudes that Greaves himself had measured. Norwood's measure is rather to be wondered at, that, notwithstanding all the difficulties he had to encounter, it should come so near to the true magnitude of a degree, than to be depended on for the exact measure of the iron standard; and is an instance of what the diligence and sagacity of a private and obscure man, unassisted by the public purse, or the contributions of friends, could accomplish; whose labours may, perhaps, be forgotten, when the measures of the French, taken under a royal commission, shall be known to the latest posterity.

The three more immediate comparisons of Greaves's measure with the Paris foot, are by the measures of Auzout, Desgodetz, and Hardy, which afford as clear a proof as can well be expected in this matter, that his measure of the iron standard was about 2 parts in 1000 deficient of Graham's London foot.

Mr. Greaves found the foot on the monument of Statilius to contain 972 such parts as his London foot contained 1000; and that on the monument of Cossutius 967 (1). These measures reduced to Graham's foot, are 970, and 965.

(1) Greaves, p. 208, 209.

Auzout's measure of the foot on the monument of M. Ebutius, is nearly equal to 973 London parts. Revillas makes this $972\frac{1}{4}$, and the Capponian foot $968\frac{3}{4}$.

The mean of these measures of the Roman foot on the marbles, is nearly 969 thousandth parts of the London foot.

Philander, in his notes on Vitruvius (2), says, the foot from a porphyry column, inscribed ΠΟΔ. Θ. exceeded the Cossutian foot by the ninth part of an inch; therefore it contained nearly 974 London parts. This column seems to have been a more authentic measure of the Roman foot, than the figures on the monuments; since the inscription shews it was intended for such, whereas they were only representations of it. And notwithstanding the inscription was Greek, the foot must have been Roman; for Philander's measure of it exceeds the mean measure of those on the marbles, by less than the sixteenth part of a London inch, and is less than some of the antient foot-rules, hereafter mentioned; whereas it wants above the third part of an inch to be equal to the Greek foot. It might, perhaps, be used by some Greek mason at Rome, to adjust his workmens rules by; for when the Romans began to be expensive in their buildings, they had most of their maçons from Greece. And though this column is not now to be found, and the accuracy of Philander's measure of it may be doubted, it is still a proof, that, at one time or other, the measure of the Roman foot was sensibly greater than that on the monument of Cossutius.

(2) P. 117. of the Lyons edit. 1552.

Lucas Pætus compared five antient foot-rules with the marbles; he seems not to have been a person of great accuracy; for though in one place he allows there was some small difference between the Statilian foot and that on the monument of Cossutius (3), yet he every-where else speaks of them as equal, and has given but one figure for the measure of both. His comparisons seem to have been made only with the latter, the measure of which he had before set up in the Capitol, as the true Roman foot (4). Three of these rules, which, he says, were exactly equal to each other, were shorter than the Cossutian foot by one seventh part of a Cossutian inch; they must then have been nearly $953\frac{1}{2}$ London parts. Of the other two, one appears to have been 958, and the other 974, equal to that from Philander's column. He decides in favour of the three shortest, to which perhaps he was induced, partly by their agreement with each other, and partly by his opinion, that the Roman foot had been continually increasing; whence it must follow, that the shortest measure of it was the most antient.

(3) Cum quo sane convenit modico discrimine hic Statilianus. Pætus de Mens. et Pond. lib. i.

(4) Greaves, p. 209. says, the Roman foot engraved by Pætus in the Capitol, agreed exactly with that on the monument of Cossutius. Pætus therefore seems at that time to have acquiesced in the opinion of Portius, which, he says, was generally received. (Leonardus Portius Vicentinus, vir sane doctus, primus omnium nostræ ætatis qui de hac re scripserit, mensuram Colotiani pedis pro vera tradidit.—Quem juniores quoque secuti sunt.) But he changed his opinion when he wrote his book, which was after he had set up the measures in the Capitol.

Greaves says, the Cossutian foot exactly agreed with some very antient and perfect Roman feet in brass; and from his comparison of two that differed from it (which seem to be the greatest and least he had met with), one of them appears to contain nearly $972\frac{3}{4}$ London parts, and the other $962\frac{3}{4}$ (5).

P. Revillas has given the measures of three foot-rules of an uncommon length; one of which contains 972 London parts, another 976, and the third 983 (6).

There are many more of these rules in being, that we have no account of; for most of the writers on this subject, preferring the authority of the marbles, have been negligent in their accounts of the foot-rules; therefore I think a mean measure from these above-mentioned of no great authority, especially as some of them have been produced only on account of their difference from the more common measure of the rest.

If a cubic Roman foot contained a quadrantal of wine, the cube of half a foot must have contained a congius, which was the eighth part of the quadrantal. Pætus, Villalpandus, and Auzout measured the standard congius of Vespasian with water; and have

(5) Greaves, p. 222. But, p. 227. he says, there is not one of ten measures of the Roman foot (besides those on the monuments), that arrive to the proportion of that deduced by Villalpandus from the congius, by 27 parts in 2000; whereas the greater of the two here mentioned contained $974\frac{1}{4}$ parts of his London foot, and he makes the foot from the congius of Villalpandus, but 986 such parts; their difference is but $2\frac{3}{4}$ two thousandth parts of the latter.

(6) Saggi di Diss. Academ. di Cortona, vol. iii.

given the weight of its contents. The two first differ from each other by almost $5\frac{1}{2}$ modern Roman ounces, which is nearly the one-and-twentieth part of the whole weight, though they both measured up to the same part of the neck. The weight of water given by Pætus, is deficient of the mean weight of Auzout's measures by 3 Troy ounces; and that given by Villoalpandus exceeds the same by 2; therefore they must both have used bad ballances.

Auzout measured this vessel twice, with the water of Trevi. His greater weight of the water is 63024 Paris grains, his lesser 62760 (7).

Picard found a cubic Paris foot of spring water to weigh 641326 Paris grains (8); and that foot containing 2089,69 cubic London inches, Auzout's greater weight will give 205,36 such inches for the solid content of the congius, his lesser 204 $\frac{1}{2}$.

According to Picard's experiment, a cubic London inch of spring water should weigh but 251 $\frac{3}{4}$ Troy grains; whereas, according to Snellius, it should weigh above 254 (9).

Our countryman Wybard made many experiments to discover the weight of water (1), from which the weight of a cubic inch of rain water should be 253 Troy grains. His experiments seem to have been made with great attention, and his inch must have been that of the iron standard, which I have already

(7) See the paper *De Mensuris*, above quoted, p. 366. 371. of the folio edit. where Picard gives these weights as Auzout's, without any farther account of them.

(8) *Ibidem*.

(9) Snellius, in *Eratoſthene Batavo*, p. 155.

(1) See Wybard's *Taëtometria*, from p. 269 to p. 287.

shewn to be less than Graham's; therefore, I think a cubic inch of Graham's measure of the water of Trevi, cannot weigh less than 253 Troy grains. This will reduce Auzout's two measures of the congius to 204,35 and 203,49 cubic London inches; which give 981,7 and 980,3 for the measure of the Roman foot.

Greaves measured the congius with millet seed (2), and comparing it with our measures of capacity, found it to contain $7\frac{1}{8}$ pints wine measure, and $6\frac{1}{6}$ pints corn measure. When he wrote, the wine gallon was universally allowed to contain 231 cubic inches (3); and the dimensions of the bushel were published yearly, by the Lord Mayor of London, to be 19 inches in diameter, and $7\frac{1}{2}$ in depth (4). But Greaves's inches were according to the iron standard in the Guildhall, and were 2 parts in 1000 shorter than Graham's. Therefore reducing these measures in that proportion, the wine gallon will be found to contain 229,62 cubic London inches, and the corn gallon 264,22. Hence by the wine measure, the congius contained 204,5 cubic London inches, and by the corn measure 203,7; which give 981,9 and 980,6 for the measure of the Roman foot.

These measures agree so well with those of Auzout, that I think a mean between them must be very near the truth, and that we may reckon the

(2) Greaves, note f. p. 303.

(3) See Wybard's *Tactometria*, p. 264. Oughtred's *Circles of Proportion*, p. 57. who quotes Gunter and Briggs for the same opinion.

(4) Wybard's *Tactometria*, p. 282.

foot from the congius to contain about 967 London parts.

Greaves took likewise another measure of the congius, by the cube of his Cossutian foot, which he found to contain 7 congii, and about an half, of millet (5). Now, the quadrantal should contain 8; hence the side of the quadrantal to this congius will contain 988 parts of the iron standard: for 988 is to 967 (Greaves's measure of the Cossutian foot) in the subtriplicate proportion of 8 to $7\frac{1}{4}$. But this is a greater measure than even that of Villalpandus, and was probably owing to an erroneous cube of the Cossutian foot; for the joiners at Rome might be as bad workmen as the scalemakers; and Greaves says, he could not get a ballance there, fit to examine the congius by weight (6).

Monf. Astruc, in his Natural History of Languedoc, says, he had caused the distance between the ninth and tenth mile-stones on the road from Nismes to Beaucaire to be measured, and that it was found to be 754 Paris toises. They were both set up by Tiberius, and the road lies in a strait line between them. This measure gives a Roman foot of 964 London parts. He does not say he took this measure himself (7), nor has he given any account how it was taken. Perhaps by a provincial measure, and

(5) Greaves, p. 225.

(6) Ibid. note f.

(7) *Memoires pour l'Histoire Naturelle de Languedoc*, p. 225. His expression is, "J'ai fait mesurer la distance de l'un à l'autre, et elle s'est trouvée de 754 toises de Paris." Where he speaks of the Marquis Maffæi, he says, "Il à mesuré;" but, in the same paragraph, speaking of himself, he again says, "J'ai fait mesurer."

after-

afterwards reduced to Paris toises; or by a wheel, which is liable to great errors: therefore we cannot allow much authority to it, unless we were better assured of its accuracy. He quotes the Marquis Maffei for a measure of 756 toises between two other mile-stones on the same road; but observes, that they were set up by different emperors, Augustus and Tiberius, and that the road formerly made an elbow between them, whose position is now not certainly known. Therefore this measure, which gives a foot of $966\frac{1}{2}$ London parts, is likewise of small authority.

All that can be determined from such uncertain and discordant data, as I have here collected, is a measure that shall probably be neither the greatest nor the least magnitude of the Roman foot. And for this, I shall take a mean from all the measures above recited, which is nearly 968 thousandth parts of the London foot.

Before I enter upon the examination of the antient buildings, it may be proper to say something concerning the nature of the evidence to be expected from them.

All buildings are planned and executed by some measure of the country where they are built. At Rome this measure was the foot, which was divided by the workmen into 4 palms, and each palm into 4 digits (8).

If

(8) Vitruvius, lib. iii. c. i. Frontinus de Agrorum Qualit. Both these authors are technical writers, and give this as the division used by workmen; and the antient foot-rules are so divided. They

If the Roman buildings were correctly executed, and we had the true dimensions of their several parts in any known measure, some divisors consisting of Roman feet, and parts of those feet, applied to these measures, must, in the same building, give the same quotient to all; and this quotient will be the measure of the foot, by which that building was constructed, in parts of the known measure. Therefore, where a range of simple divisors, applied to the principal parts of any building, give as nearly the same quotient as can be expected from the common inaccuracies of workmanship, we may reasonably conclude, that these divisors were the architects numbers; and the foot derived from them, that by which the building was constructed.

As an architect cannot be supposed to be limited to a few digits in the extent of the front, or of the depth of large buildings, it is probable such measures consisted of whole feet. These, and the diameters of circular buildings, I call prime measures.

In all large prime measures, the preference is to be given to a round number for the divisor; as it is more probable a building should be designed for 100 feet in front, than for 99 or 101: and because the passus was 5 feet, I reckon any multiple of 5 a round number.

The diameters of columns are of less authority than any other horizontal measures; not only on account of the difficulty of measuring them correctly,

They both mention the duodecimal division, which seems to have been used by the vulgar; for the Romans divided every integer into 12 unciae.

but because errors of workmanship, to which they are more liable than square members, more sensibly affect the magnitude of the foot in small measures, than in large ones.

Uprights, of any considerable height, are of less authority than horizontal measures, from the difficulty of taking them correctly; and being designed by modules, few of them answer well to the foot measure. But here we must except such shafts of columns as are of one block of marble; which seem to be as good authority as any part of a building: for the necessity of making them all exactly of the same length, must produce accuracy; and the doing this was no difficult piece of workmanship. Being likewise commonly (if not always) wrought at the quarry, to save expence in the carriage, they were probably bespoke to some simple measure; and we shall find all such shafts answer to some number of whole palms.

In the following enquiry, I suppose that Desgodetz's Paris foot contained 1065,4 such parts, as our London foot contains 1000: since it appears, from the comparison of his measure of the door-case of the Pantheon with Greaves's, that if Greaves's London foot was not above 2 parts in 1000 shorter than Graham's, Desgodetz's Paris foot was but $\frac{3}{10000}$ of the London foot shorter than that in the archives of the Royal Society. This difference (if such there was) does not amount to the 277th part of an inch; which is much nearer than we can expect to find the measure of the Roman foot, from all the remains of antiquity now in being.

The

The temple of Fortuna virilis.

When this temple was built, is not known; but it is commonly thought, from the poorness of the materials, and the style of the architecture, to be one of the most antient buildings now remaining in Rome. The great simplicity of its parts, and their near agreement with each other, afford so clear an evidence of the measure by which it was constructed, that I have chosen to begin with it, as an introduction to the rest.

It has four Ionic columns in front, and seven in depth.

The depth, taken between the centers of the angular columns, is 54 feet $8\frac{1}{4}$ inches Paris measure, equal to 58264,06 thousandth parts of the London foot; which contain just 60 Roman feet of 971 $\frac{1}{2}$ such parts each.

The distance between the centers of the columns in this range, is 9 feet $1\frac{3}{8}$ inches, equal to 9710,6 London parts; containing 10 Roman feet of the same measure with the former.

The extent of the front, taken between the centers of the angular columns, is 28 feet $8\frac{1}{6}$ inches, = 30556,25; which contain $31\frac{1}{2}$ Roman feet of 970 $\frac{1}{2}$ London parts each.

The distance between the centers of the angular columns, and those on each side the entrance, is exactly equal to the distance between the centers of the columns on the sides; being 10 Roman feet of 971 $\frac{1}{2}$ parts each.

There remain therefore $11\frac{1}{2}$ Roman feet, for the distance between the centers of the columns on each

side the entrance. This measures 10 feet $5\frac{1}{2}$ inches (9), equal to 11134.9; which, divided by $11\frac{1}{2}$, gives $968\frac{1}{4}$ for the Roman foot.

This disagreement between the measures in the front, cannot be reconciled by any probable divisors; therefore it must be owing to an error in workmanship. And if the distance between the centers of the middle columns were but a little more than $\frac{3}{8}$ of a London inch greater, both that, and the whole extent of the front, would answer to the same measure of the foot with the sides. Now, as the middle intercolumniation was intended to be greater than the rest, the workman might be less exact in laying out the front, than the sides, where they were all to be equal; for the front intercolumniations next the angles being kept equal to those on the sides, the symmetry of the building would be preserved, and the whole error fall on the middle intercolumniation, where it could not be discovered.

Or if, on the other hand, we suppose the measure of the extent in front to have been correct, and the error to have lain in the sides, the workman must still have made the intercolumniations on the sides equal to each other, and those in the front next the angles, equal to them: and, in this case, 970 would be the true measure of the foot, instead of 971.

Therefore, I think, it cannot be doubted, that one or the other of them was the measure by which this building was constructed.

(9) Desgodetz has figured this 10 feet $5\frac{1}{2}$ inches; which will be found to be a mistake, by comparing the correspondent numbers, and by his measure of this interval in modules.

The temple of Vesta, at Rome.

This building is remarkable for the style of its architecture, and the elegance of its workmanship. The pointed abacus shews the architecture to be Greek; which is a strong presumption in favour of its antiquity, as it is the only example of the kind now remaining at Rome (1).

None of the diameters will answer to the same measure of the foot with the other parts, by probable divisors. This was likely enough to be the case, if the circles of the plan were described by a cord, whose stretching might increase their diameters beyond the intended measures. But the rest of the measures agree pretty nearly with each other, by as simple divisors as can be expected in so small a building.

At the door-case, the arch of the wall is discontinued, and the groundfil is strait. The width between the jambs is 9 feet $1\frac{2}{3}$ inches, equal to 9736,6; which, divided by 10, gives a foot of 973,7 London parts.

From the wall to the centers of the columns, is 8 feet $8\frac{1}{2}$ inches, = 9244,6; which, divided by $9\frac{1}{2}$, gives a foot of 973,1 parts.

From the wall to the extremity of the basement, is 10 feet 6 inches, = 11186,7; which, divided by $11\frac{1}{2}$, gives 972,7.

(1) That is to say, it is probably older than the Pantheon; for, according to Pliny, in his thirty-sixth book, there seems to have been no marble temple in Rome above sixty years older than that building. And his history of marble is confirmed by the saying of Augustus, "That he found Rome brick, and left it marble." Sueton. in Aug. c. 28.

From the top of the surbase within, to the pavement of the cell, is 7 feet $9\frac{1}{8}$ inches, = 8271,65; which, divided by $8\frac{1}{2}$, gives 973,1.

From the top of the surbase without, to the outer pavement, is 9 feet $1\frac{5}{8}$ inches, = 9751,4; which, divided by 10, gives 975,1.

The shafts of the columns are 27 feet 5 inches, = 29209,8; which, divided by 30, gives 973,7.

The columns, with their bases and capitals, are 32 feet 0 inch, = 34092,8; which, divided by 35, gives a foot of 974 London parts.

The mean measure of the foot from this building, is 973,6; which agrees nearly with the foot of Philander's porphyry column above-mentioned, and with one derived from Greaves's measure of the monument of Cestius, whose side within the city (as he says in p. 151 of his works) is completely 78 feet English; which, reduced to Graham's measure, is 77,84, and contains 80 Roman feet of 973 London parts each.

The temple of Vesta, at Tivoli.

The measures of the parts of this building disagree so much with each other, by any probable divisors, that I shall only mention the width of the door-way between the jambs, which is 7 feet $3\frac{3}{4}$ inches, = 7790,7, and answers to 8 Roman feet of 973,8 parts each.

The Pantheon.

This is a circular building, with a portico before the entrance; which, having been added after the body of the work was finished (2), has induced many

(2) See Desgodetz, p. 3. & p. 14.

to think the temple was more antient, and that Agrippa only adorned it with marble, and added the portico. But the inscription on the freze, " M. " Agrippa L. F. Cos. tertium fecit," seems to refer to the whole. And Pliny, enumerating the most magnificent buildings in Rome, says, " Pantheon " Jovi ultori ab Agrippa factum; cum Theatrum " ante texerit Romæ Valerius Ostiensis architectus, " ludis Libonis (3)." If the latter part of this passage alludes to the dome of the Pantheon (as it seems to do), it will imply, that Agrippa built it. In another place (4), Pliny calls this building Agrippa's Pantheon. Now, temples of this form were usually peripteres; that is to say, encompassed with columns (5), as the two temples of Vesta; but such a colonnade to this building (supposing the entablature to reach up to the top of the wall) could not consist of fewer than twenty-six columns of 9 London feet in diameter; which, with their entablature, must have cost much more than the whole building, as it now stands. Therefore, it seems as if no columns were at first intended on the outside, and that the portico was an after-thought.

This temple is said to have been burnt in that great conflagration, which happened in the reign of Titus (6); and again by lightning, in the thirteenth year of Trajan (7), and to have been restored by

(3) Pliny, lib. xxxvi. c. 15.

(4) Ibid. c. 5.

(5) Vitruvius, lib. iv. c. 7.

(6) Xiphilin. in Tito. Suetonius likewise mentions this fire; but does not say what buildings were burnt by it. Titus, c. 8.

(7) Eusebius in Chronico.

Adrian (8). But it does not seem to have been totally destroyed either time; for the inscription of Severus says, it was "*vetustate corruptum*;" which could hardly be true, if it was entirely rebuilt by Adrian, who began his reign but seventy-six years before Severus; but is very applicable to a building that had stood above 200 years, and been twice damaged by fire. Neither does it seem possible, that a building of this size and solidity, without any timber in it, except the roofing of the portico, should be totally consumed by the fire of the neighbouring buildings. Therefore, we may suppose the measures of the principal parts of it to be according to the foot of the Augustan age.

If the execution of this noble building had been answerable to the beauty of the design; the multiplicity of its parts, and the accuracy with which Desgodetz has measured them, would have afforded ample materials for the discovery of the foot by which it was constructed: but the dimensions of such parts of it as should be equal to each other, differ so widely, that scarce any but the prime measures are to be depended on.

These were, probably, the diameter of the circle passing through the centers of the columns within; for on this, the diameter of the cupola depended; and the diameter of the whole building, from out to out.

Desgodetz has given twelve measures of the diameter within the shafts of the columns. The mean from them all, is 133 feet $2\frac{2}{7}$ inches Paris measure:

to this, add the mean diameter of the columns, 3 feet $4\frac{4}{7}$ inches, their sum, 136 feet $7\frac{1}{7}$ inches, equal to 145533,6 London parts, is the diameter of the circle passing through the centers of the columns; which, divided by 150 (as round a number as can be desired), gives 970,2 such parts for the measure of the Roman foot.

The mean depth of the four long chapels, between the shafts of the columns and the wall, is 9 feet $1\frac{4}{8}$ inch, = 9764,3; which, divided by 10, gives a foot of 976,4 London parts; but by $10\frac{1}{4}$, one of 952,6; which is certainly too small. And if this was an arbitrary measure, we cannot admit the fraction of a palm in it. Therefore, we may suppose these chapels were intended to be 10 Roman feet deep in the clear between the columns and the wall.

The mean diameter of the columns, 3 feet $4\frac{4}{7}$ inches, = 3622,36 London parts, divided by $3\frac{3}{4}$, gives a foot of 966 parts. The shafts of these columns measure 27 feet $1\frac{1}{3}$ inches, whose eighth part (reckoning the shaft at 16 modules) is 3 feet $4\frac{2}{3}$ inches; which is somewhat less than the mean of their diameters. Yet even this measure (which is equal to 3610,5 London parts) does not want half a Roman digit of $3\frac{3}{4}$ feet of 970,2 parts each; which the architect might safely neglect, and allow $3\frac{3}{4}$ feet for these diameters, in his general plan.

Now, if to twice the depth of these chapels, or 20 Roman feet, we add twice the semidiameter of the columns, or $3\frac{3}{4}$ such feet; and to their sum, the diameter of the circle passing through the centers of the columns, 150 feet; we shall have $173\frac{3}{4}$ Roman feet

feet for the diameter from wall to wall of the opposite chapels.

Desgodetz has given four measures of this diameter, whose mean, 158 feet $2\frac{2}{3}$ inches, is equal to 168570 London parts, which, divided by $173\frac{3}{4}$, give 970,2 —, for the measure of the Roman foot; agreeing with that found from the diameter of the circle passing through the centers of the columns.

Or if, instead of taking a mean of the four diameters from wall to wall, we add twice the mean depth of these chapels, and twice the mean semi-diameter of the columns, to the mean diameter of the circle passing through their centers, we shall have 158 feet 4 inches for the diameter from wall to wall, equal to 168688 London parts; which, divided by $173\frac{3}{4}$, give 970,9, for the measure of the foot.

But if this diameter from wall to wall was a prime measure, as well as that of the circle passing through the centers of the columns, the depth of these chapels must have been a residuary measure, and might be more or less than 10 Roman feet. And if we divide the diameter last mentioned by 174 (which being composed of 150, and twice 12, may here be reckoned a round number), it will give a foot of $969\frac{1}{2}$ London parts. But if, instead of twice the mean depth of these chapels, we take twice the greatest depth, 18 feet 6 inches, for part of this diameter, the same divisor will give a foot of $970\frac{1}{2}$ such parts.

The measure of the diameter from out to out cannot be so well determined as either of the former; for Desgodetz has given but one measure of the outer wall, which is 5 feet $4\frac{1}{2}$ inches; to this we must add 1 foot $10\frac{1}{3}$ inches, for the mean depth of the niches

in

in the long chapels; and twice their sum, 14 feet $5\frac{2}{3}$ inches, added to 158 feet $2\frac{2}{3}$ inches, the inside diameter between the walls of these chapels, will give 172 feet $8\frac{1}{3}$ inches for the diameter from out to out. This is equal to 183988,6 London parts; which answer to 190 Roman feet, of 968,4 such parts each. But this diameter being got by the addition of many small parts, one of which is a single measure, is of small authority. Though if the outer wall is 5 feet $4\frac{1}{2}$ inches thick in brick-work, and the backs of the niches in the long chapels are incrufted with marble (as I suppose they are), a proper allowance for the incrustation will make this diameter answer to a foot of 970 London parts.

The principal parts of the circular range within, are too unequal to answer to any regular division into compartments; and the two largest measures in it sufficiently evince, how little use is to be made of the rest: for the width of the grand chapel opposite the entrance, and the width of the entrance between the pilasters in the range, will not answer to the same measure of the foot, by any probable divisors.

But the inequality of the faces of the pilasters, that terminate the solids in this range, affords the clearest proof, that no certain measure of the foot is to be obtained from the lesser parts of this building: for two of them, at the extremities of the same solid, differ in breadth by a Paris inch and three quarters. One of the two on each side the grand chapel exceeds the other in breadth, by a Paris inch and a quarter; and it exceeds the least in the circular range, by no less than $4\frac{1}{4}$ such inches, though the diameter of the column before it is less than that of some in the range.

The diameter of the circular light at top is 27 feet 5 inches, equal to 29209,7; which, divided by 30, gives $973\frac{2}{3}$ for the Roman foot. Whether Desgodetz took more measures than one of this diameter, does not appear, nor how he took it. It is probable, that 30 Roman feet was the measure here intended, being a round number, and one fifth of the diameter between the centers of the columns: but the making this light a true circle, precisely answering to a given measure, was a difficult piece of workmanship. Therefore, this measure is of small authority.

The square compartments on the pavement measure 9 feet $0\frac{2}{3}$ inch, or 9647,79 London parts; which, divided by 10, give a foot of 964,8 parts. But Desgodetz observes (9), that this is not the original pavement: and this measure of the foot answers to the age of S. Severus, as I shall shew hereafter.

This pavement being considerably raised above the old one, no measures of uprights taken from it are of any authority.

The height of the attic, above the cornice it stands upon, is 27 feet $2\frac{3}{4}$ inches, equal to 29010; which, divided by 30, gives a foot of 967 parts. But the walls of this building being brick, this measure may have been diminished by their settling, as the whole error occasioned by it might be made to fall on this part, rather than on the order below.

The length of the shafts of the columns in the circular range, is 27 feet $1\frac{1}{3}$ inches, = 28884,18; which answers to $29\frac{3}{4}$ feet, of 970,9 parts each.

(9) Desgodetz, p. 41.

The width of the door-case, between the jambs, has been supposed, by Picard, De la Hire, Eifenschmid, and others, to have been a measure of whole feet; for this reason, because they thought it an arbitrary one, and therefore not likely to contain a fraction (1). But in this, I think, they were mistaken; for it appears to have been determined by the width of the passage within, of which it takes up just three fourths. The proportion is too simple, and too exactly executed, to be accidental; and it is highly improbable, that the width of this passage, which is a principal part of the plan of the inside, should be determined by that of the door-case. The width of the passage, is 24 feet $5\frac{2}{3}\frac{3}{4}$ inches, = 26098,6; that of the door-way, next the passage, is 18 feet $4\frac{7}{8}$ inches, = 19610; next the portico, 18 feet $4\frac{1}{3}$ inches, = 19561,9; none of which will give a measure of the foot, answering to the rest of the building, by whole divisors. The width of the door-way next the portico (which has been supposed to be just 20 Roman feet), divided by 20, gives 978. But the diameters of the temple (taking either the greatest or the least, instead of a mean), will not answer to a foot of this measure, by probable divisors.

The principal measures in the portico are, its extent in front and depth, and the shafts of the columns.

(1) Mr. Greaves says, this door-case is one entire marble stone. See p. 348. and p. 494. And Ficoroni (a late writer), in his *Vestigia di Roma antica*, p. 132. speaking of this door-case, says, “Essendo amendue i lati, col di sopra, e il di sotto di un solo pezzo di marmo.” But Desgodetz, p. 18. says, that each side of the architrave consists of two stones, and the top, of one; and corrects Serlio, for saying the whole was of one piece.

The extent in front, taken between the centers of the angular columns, is 98 feet $4\frac{3}{8}$ inches, equal to 104797,6; which contain 108 Roman feet of 970,3 parts each. Its depth on the right-hand side of the entrance (2), from the center of the angular column to the center of the pilaster, is 41 feet $5\frac{1}{8}$ inches, = 44136,4, and contains $45\frac{1}{2}$ feet of 970 + parts. The shafts of the columns, which are each of one single stone, measure 36 feet $7\frac{7}{8}$ inches (3), equal to 39053,6; which answer to $40\frac{1}{4}$ Roman feet of 970,3 London parts.

I can find no measures in this building of equal authority with the diameters, the shafts of the columns in the circular range, and these measures in the portico. All of which give the measure of the Roman foot greater than 970 such parts as the London foot contains 1000.

In the rest of the buildings, where the measures do not require a particular explanation, I shall first give Desgodetz's measures, with their reductions to thousandth parts of the London foot, and then set down the divisors and quotients with two hooks between them; the divisor on the left-hand, expressing

(2) By some expressions of Desgodetz, his plans seem to be inverted, as gravings commonly are. I speak according to the plan, supposing the reader to view it in front.

(3) Desgodetz, p. 19. has figured the shafts of the pilasters in the portico 36 feet $7\frac{1}{2}$ inches, and in modules 16 mod. $8\frac{1}{2}$ parts; which is somewhat too great for his foot measure, his module being 27 Paris inches. But p. 16. he has figured the shafts of these columns 16 mod. $8\frac{3}{4}$ parts, which is 36 feet $7\frac{3}{8}$ inches. And that this is no mistake, appears from his measures of the capitals of each.

a num-

a number of Roman feet; and the quotient on the right, their magnitude in parts of the London foot.

The temple of Peace.

The length on one side within the walls, including the pronaus or porch, is 269 feet $8\frac{1}{2}$ inches, =	
287347,26 —————	296) ... (970,8 —
The width of the nave 77 feet $5\frac{1}{2}$ inches, = 82524,1 (twice) ———	
	85) ... (970,9 —
The width of the side-building 48 feet 0 inch, = 51139,2 (thrice) ———	
	52 $\frac{3}{4}$) ... (969,5 —
The length of the first division of the side 71 feet 5 inches, = 76087,3	
	78 $\frac{1}{2}$) ... (969,3 —
The length of the second division 71 feet 8 inches, = 76353,87	
	78 $\frac{3}{4}$) ... (969,6 —
The length of the third 70 feet $3\frac{1}{2}$ inches, = 74888,75 ———	
	77 $\frac{1}{4}$) ... (969,4
The width of two of the passages in the cross walls, is 22 feet 4 inches, = 23793,9 ———	
	24 $\frac{1}{2}$) ... (971,2 —
The width of the exedra in the middle division of the side 53 feet 11 inches, = 57442,82 ———	
	59 $\frac{1}{4}$) ... (969,5
Its depth is equal to the width of the passages in the cross walls.	
The two middle entrances through the porch, 15 feet 3 inches, =	
16247,35 ———	16 $\frac{3}{4}$) ... (970 —
The side entrances 14 feet $4\frac{1}{2}$ inches, = 15315,13 ———	
	15 $\frac{3}{4}$) ... (972,4 —

The entrance into the porch on the
right-hand 14 feet 4 inches, =

15270,74

15 $\frac{3}{4}$) ... (969,5

The shaft of the column (of one stone)

49 feet 3 inches, = 52470,9

54) ... (971,7 —

The diameter of the column 5 feet

8 $\frac{1}{4}$ inches, = 6058,47

6 $\frac{1}{4}$) ... (969,4 —

The height of the order 71 feet

0 $\frac{1}{2}$ inch, = 75687,79

78) ... (970,4

A mean from these twenty-one measures gives a
Roman foot of 970,3 parts. But the measures in
this building seem to be very incorrectly executed.

The temples of Bacchus, and of Faunus.

I shall not particularly recite the measures of these
buildings, because they seem to be the works of a
much later age than any of the rest. By the different
workmanship of the shafts of the columns in the
temple of Bacchus (4), it seems to have been com-
piled out of the ruins of other buildings. In the
temple of Faunus, most of the columns are of gra-
nate, but some of them of white marble; the cross
wall was evidently designed to support a flat roof,
the outer wall being too weak to bear a cupola (5);
and the windows (which I take to be as old as the
rest of the building) are in the Gothic style: all
which are marks of a late age. Therefore both
these temples seem to be more modern than they are
commonly thought to be. The temple of Bacchus

(4) See Desgodetz, p. 68.

(5) Ibid. p. 79.

answers to a foot of 968 parts, that of Faunus to one less than 967.

The amphitheatre of Vespasian.

The plan of this building is elliptical; and the area in the middle being allotted to the exhibition of the shews, seems to be the part on which the magnitude of the whole depended. Therefore one of its axes must be the prime measure.

The greater axis measures 263 feet 11 inches, = 281176,8 London parts; the lesser 165 feet 1 inch, = 175879,7. These numbers are so nearly in the proportion of 8 to 5, that there is little reason to doubt their being intended for it.

The only two whole numbers in this proportion that will answer our purpose are 288 and 180, by which the greater axis will give 976,3 London parts for the measure of the foot; the lesser, 977,1.

Either the distance from the inner to the outer wall, or that from the inner wall to the extremity of the upper step, was probably a prime measure too. The former is 155 feet $5\frac{1}{2}$ inches, = 165610,5; which, divided by 170, gives 974,2. The latter is 158 feet $0\frac{1}{2}$ inch, = 168340,6; which, divided by 172, gives 978,7.

But the axes will admit of other divisors, which are in my opinion as probable at least, as those above-mentioned. For if we suppose the greater axis to have been the prime measure from which the lesser was determined in the proportion of 5 to 8, and assume 290 Roman feet for it (which is a more probable measure than 288), the lesser axis must then be $181\frac{1}{4}$ such feet. And the measure of the foot by the

the greater axis will be 969,6 parts; by the lesser 970,3.

The distance from the inner to the outer wall will not answer to this measure of the foot by any whole divisor. And that from the inner wall to the extremity of the upper step, divided by 174, gives but $967\frac{1}{2}$. But this disagreement will not appear extraordinary, when we consider that these distances are not single measures, but the sums of nine or ten smaller ones (6), and may therefore be liable to an error of some inches.

The lesser parts are so incorrectly executed, that no use can be made of them. And the heights of the four orders, and of their parts, are so irregular as not easily to be accounted for.

The amphitheatre at Verona.

The principal measures in this building answer to the Greek foot; which is not the subject of the present inquiry.

The arch of Titus.

This edifice is so ruined, that we have not the measure of the extent of its front.

The depth of the solid is 14 feet		
7 inches, = 15537,1 —	—	16)... (971,1 —
The width of the entrance, 16 feet		
4 inches, = 17401,5 —	—	18)... (966,7
The shafts of the columns, 16 feet		
2 inches, = 17223,97 —	—	17 $\frac{3}{4}$)... (970,9

(6) See the section, Pl. 3.

The arch of Constantine.

The measures of this elegant design are so incorrectly executed, that I can make no use of them. The fronts of the two outermost solids differ by a Paris inch and one third; the side entrances by $1\frac{2}{3}$; the distances of the columns from the pilasters (a measure of little more than 18 inches) differ an inch and $\frac{1}{2}$; and the depth of the solid at one end, exceeds that at the other end by no less than $7\frac{1}{4}$ Paris inches.

The temple of Antonine and Faustina.

Desgodetz has given the measure of no part of this building in feet and inches, but the diameters of the columns; which he says do not differ from each other, as in many of the rest.

The diameter of the columns is 4 feet

$6\frac{1}{2}$ inches, $(= 4846,09. \text{---} 5) \dots (969,2$

The extent of the portico taken be-

tween the centers of the angular

columns, 25 mod. $16\frac{5}{6}$ parts, $=$

$61935,7 \text{---} 64) \dots (967,7$

The shafts of the columns, 15 mod.

28 parts, $= 38607,2 \text{---} 40) \dots (965,2$

These shafts were probably intended for 16 modules, and their measure is more to be relied on than that of the diameters, both on account of its size, and because the correct execution of it was not only more easy, but more necessary. Therefore, though these measures are too few, and too unequal to determine the exact magnitude of the foot used in the con-

struction of this building, yet it seems to have been less than any we have hitherto met with.

The arch of Septimius Severus.

This building deserves our particular attention, both on account of the correctness of the workmanship, and because it exhibits a different measure of the Roman foot from any of the preceding. It is all of white marble. The stones are laid without mortar, and fastened together with cramps. And the shafts of the columns are each of one stone.

The whole extent in front, is 71 feet

$5\frac{5}{8}$ inches, = 76161,3 ——— 79) . . . (964,1

The depth at one end, is 21 feet

$8\frac{3}{4}$ inches, = 23150,3 ——— 24) . . . (964,6

The depth at the other end, 21 feet

$8\frac{5}{6}$ inches, = 23157,6 ——— 24) . . . (964,9

The width of the middle entrance,

20 feet 10 inches, = 22195,8 23) . . . (965

The width of the side entrances,

9 feet 2 inches, = 9766,17 ——— 10 $\frac{1}{8}$) . . . (964,5

From the middle entrance to the extremity of the outer solid, on one side, is 25 feet $3\frac{3}{4}$ inches,

= 26967,9 ——— 28) . . . (963,1

The like on the other side, 25 feet

$4\frac{1}{2}$ inches, = 26997,5 ——— 28) . . . (964,2

The distance between the shafts of the outermost columns, is 60 feet $7\frac{5}{6}$ inches,

= 64619,5 — (7) 67) . . . (964,5

The

(7) This and the two following measures are inserted only to shew the probability, that the diameters of the columns were designed

The distance between their centers,
 63 feet $4\frac{1}{3}$ inches, = 67504,9 70)...(964,4
 The distance including their shafts,
 66 feet $0\frac{2}{3}$ inch, = 70390,4 ——— 73)...(964,3
 The diameter of the columns, 2 feet
 $8\frac{1}{2}$ inches, = 2885,46 ——— 3)...(961,8

The uprights will not answer to any one measure of the foot by whole divisors; nor will they agree with each other by broken ones, so nearly as the horizontal measures do, which may partly be owing to the difficulty of taking them correctly.

The shafts of the columns measure
 21 feet $8\frac{1}{4}$ inches, = 23105,9 24)...(962,7
 The pedestals, 12 feet $3\frac{1}{12}$ inches,
 = 13088,14 ——— 13 $\frac{1}{2}$)...(969,5
 The whole order, 33 feet $4\frac{1}{4}$ inches,
 = 35535,53 ——— 37)...(960,4
 The attic, 15 feet $5\frac{1}{3}$ inches, =
 16454,51 ——— 17)...(967,9
 The blocking, 1 foot $9\frac{2}{3}$ inches, =
 1923,64 ——— 2)...(961,8
 The whole height, 62 feet $10\frac{2}{3}$ inches,
 = 67001,8 ——— 69 $\frac{1}{2}$)...(964,1 —

I have produced these uprights, rather as an instance how little such measures are to be depended on, even in buildings correctly executed, than as of any authority for the measure of the foot. For they cannot be reconciled to a common measure without

signed to be three Roman feet, and not as of any authority in the determination of the measure of the foot.

fractions of the digit in the divisors, by which means any measure of the foot might be derived from any building.

A mean from the seven first horizontal measures gives a Roman foot of 964,3 parts. A mean from the uprights one of 964,4.

But the depth of the solid, and the widths of the entrances are most to be depended on, and these give a foot of $964\frac{3}{4}$ London parts; which will be found to be the greatest measure this building will answer to by probable divisors.

For though the extent of the front, or the width of the middle entrance, must probably have been the prime measure in a building of this kind; yet no part seems more likely to consist of some number of whole feet (or at least whole palms) than the depth of the solid; since there appears no reason why the architect should use odd digits in the measure of it. Now if we suppose the extent of the front to have been $78\frac{1}{2}$ Roman feet, they will contain 970 parts each; but to make the depth agree with this measure, it must have been $23\frac{7}{8}$ such feet, which does not seem probable, for the reason above-mentioned. The middle entrance must then have been $22\frac{7}{8}$, instead of 23 whole feet, and the extent on each side $27\frac{1}{6}$, instead of 28. And there will not be one whole measure in the plan of this building; which is improbable. Neither do these broken measures serve for the subdivision of the sides better, or even so well as the whole ones.

The shafts of the columns are so nearly 16 modules, that they seem to have been designed for that proportion; and $\frac{1}{32}$ of a Paris inch added to the diameters

meters would make them so. But this is not sufficient to make those diameters answer to 3 feet of $964\frac{3}{4}$ parts, of which they want almost $\frac{1}{10}$ of a Paris inch. This may be partly an error in the workmanship, and partly in Desgodetz's measure; but if this quantity were added to them, they would not only answer to the same foot with the rest of the building, but all the parts in which their measure is included, would agree better with the depth of the solid, and the widths of the entrances.

The height of the blocking was probably intended for 2 Roman feet, though it answers to no greater measure of that foot than the diameters of the columns do. But measures of 2, 3, and 4 feet, seldom agree with the larger ones in any of the buildings; for a small error, either in the workmanship, or in the measuring, makes a very sensible difference in the length of the foot derived from such intervals. And this blocking does not want the $\frac{1}{13}$ part of a London inch, of 2 feet of 965 parts each.

The bankers arch, and the portico of Severus, are contemporary buildings with this, and seem to agree with it in the measure of the foot. The former is a very small building, and its parts will not answer to any one common measure by divisors of whole digits. However, the depth of the solid (which, as I have before observed, is most likely to be a whole measure) taken at three different parts, is 6 feet 4 inches, which answer to 7 Roman feet of 963,93 London parts.

There are but few measures in the portico of Severus. Those most to be depended on are, the whole extent in front, and the shafts of the columns, which
are

are each of one block. The former measures 56 feet $11\frac{78}{100}$ inches, = 60708,3 —, which contain 63 Roman feet of 963,6 parts each. The latter measures 26 feet $11\frac{1}{4}$ inches, = 28699,22, which contain $29\frac{3}{4}$ such feet of 964,7 parts. The whole column, with the base and capital, measures 32 feet $6\frac{1}{2}$ inches, = 34669,9 London parts, which contain 36 Roman feet of 963 parts each.

It appears by the inscription on the architrave of the portico of the Pantheon (8), that Severus repaired that temple; and Desgodetz observes, that the pavement is not so old as the building. That it was laid by Severus is probable, from the correctness of its measures, which is a remarkable characteristic of the workmanship of that age. The square compartments on this pavement answer to 10 Roman feet, of 964,78 parts, agreeing with a mean from the depth of the solid of this arch, and the widths of the entrances, which are the most unexceptionable measures in it. The breadth of the strait bands between these compartments, is equal to the measure Desgodetz has given of the diameters of the columns of this arch, and answers to 3 feet of 961,8 parts. But these bands might be purposely made somewhat short of the full measure, to leave room for one of the same breadth with the rest between the outermost compartments and the circular range; for notwithstanding this, the workmen found themselves obliged to diminish both the outermost compartment, and the band beyond it.

(8) Imp. Cæs. Septimius Severus——et Imp. Cæs. M. Aurelius Antoninus ——Pantheum vetustate corruptum cum omni cultu restituerunt.

The concurring testimonies of these contemporary works, though not of sufficient authority to establish any measure of the foot by themselves, are yet very strong evidence in favour of the measure above derived from this arch. And we shall find a farther confirmation of it in Diocletian's baths.

The baths of Diocletian.

The whole length of this building is
439 feet 3 inches, = 467977 485)... (964,8

The length of the great hall, 180 feet
8 $\frac{1}{6}$ inches, = 192497 + — 200)... (962,5

The breadth at one end, 74 feet 3 $\frac{5}{8}$
inches, = 79179,9 — — 82)... (965,6

Its breadth at the other end, 74 feet
5 $\frac{1}{2}$ inches, = 79327,9 — — 82)... (967

The distance between the middle so-
lids on the side, 49 feet 8 $\frac{1}{4}$ inches,
= 52937 + — — 55)... (962,5

The depth of the recess out of the
great hall between these solids, is
33 feet 6 $\frac{1}{6}$ inches, = 35705,7 37)... (965

The distance between one of these
solids and the nearest angular solid,
32 feet 11 $\frac{5}{8}$ inches, = 35124,9 36 $\frac{1}{2}$)... (962,3

The distance between the other of
them and the other angular solid,
33 feet 1 $\frac{1}{3}$ inch, = 35276,57 36 $\frac{1}{2}$)... (966,4

Entrance at one end of the hall,
36 feet 2 $\frac{1}{6}$ inches, = 38546,8 40)... (963,7

Entrance at the other end, 36 feet
1 $\frac{1}{4}$ inch, = 38465,4 — (twice) 40)... (961,6

Passage

Passage from the circular vestibule,

30 feet $9\frac{1}{4}$ inches, = 32783,2

(twice) ————— 34)...(964

Side entrance into the vestibule, 20

feet $9\frac{3}{4}$ inches, = 22166,2 ————— 23)...(963,7

A measure from 4 feet 6 inches to

4 feet $6\frac{1}{2}$ inches, repeated 12 times,

the mean from all of them is 4 feet

$6\frac{7}{8}$ inches, = 4818,03 ————— 5)...(963,6

A measure from 5 feet 5 inches to

5 feet $5\frac{1}{4}$ inches, repeated 11 times,

the mean from all is 5 feet $5\frac{2}{11}$

inches, = 5787,05 ————— 6)...(964,5

A measure from 2 feet $8\frac{1}{2}$ inches to

2 feet $8\frac{5}{6}$ inches, repeated 4 times,

the mean from all is 2 feet $8\frac{7}{12}$

inches, = 2892,86 ————— 3)...(964,3

A multiple of 6 feet on the wall,

where this measure of 3 feet is

thrice repeated, is 43 feet $5\frac{1}{8}$ inches,

= 46267,2 ————— 48)...(963,9

The diameter of the circular vestibule is too principal a part to be neglected. It answers to 65 Roman feet of 969,1 parts, which the other measures in this building will not come up to by probable divisors. It answers likewise to $65\frac{1}{4}$ feet of 965,4 parts; but an odd palm in so large a measure is not probable. Therefore it seems to have been incorrectly executed, and perhaps this circle was described by a cord.

The

The shafts of the columns, each of
 one block, measure 35 feet $11\frac{5}{12}$
 inches, = 38302,6 ——— 39 $\frac{3}{4}$)... (963,6
 The whole column is 43 feet $6\frac{1}{8}$ inches,
 = 46359,7 ——— 48) ... (965,8

These are the principal measures in this building,
 and a mean taken from all of them, except the dia-
 meter of the vestibule, gives a foot of 964,1 London
 parts.

But the three small measures, of 5, 6, and 3 Ro-
 man feet (the first of which is the passus) are alone
 sufficient to discover the measure of the foot by which
 this building was constructed. For, being so often
 repeated with so little variation, they must afford a
 mean very near the truth, if their divisors be rightly
 assumed; and that these are so, can hardly be doubted.
 For, were the two largest to be encreased but by a
 single digit, they would give different measures of
 the foot from each other; and that from the passus
 so encreased, would fall short of 952 London parts;
 which is less than the least of the antient foot-rules.
 On the other hand, were they diminished by the
 same quantity, they would not agree together so
 nearly in the measure of the foot as they now do,
 nor give such a measure of it as the rest of the build-
 ing will answer to by probable divisors. Besides, I
 have here reckoned only such of these measures as
 Desgodetz has figured on his plan; whereas they are
 much oftener repeated, and that not in the diameters
 of columns, or the members of an order, but in
 breaks, projections, and thickneses of walls, where
 a digit or two more or less could make no visible
 difference,

difference, provided the correspondent parts were made to answer to each other: and it is highly improbable that such odd measures as the passus wanting a digit, 6 feet wanting the same, and 3 feet wanting half a digit, should be so often repeated in so large a building, and in places that do not seem to require any such precise exactness. Therefore these measures must have been whole feet, and the divisors are rightly assumed.

Their mean is 964,¹ London parts, which exactly agrees with that found above from all the measures taken together.

The other remains of antiquity contained in this work are such fragments as can be of little use in the present enquiry.

C O N C L U S I O N.

It appears from the measures of these buildings, that the Roman foot before the reign of Titus exceeded 970 parts in 1000 of the London foot, and in the reigns of Severus and Diocletian fell short of 965.

Whether this difference proceeded from any alteration in the standard, or from a false measure of it being got into common use, either before the reign of Titus or after, is uncertain.

We have no account of any alteration made by law in the Roman standards after the Plebiscitum Silianum, quoted by Festus (9); but as great a difference as this might arise from their having been lost or destroyed.

(9) Under the words *Publica pondera*.

They were kept in the Capitol (1), and Rigaltius, from a passage in Hyginus, observes that the standard of the foot was deposited in the temple of Juno Moneta (2). Now the Capitol was burnt no less than three times; first in the civil war of Sylla (3); then again when Sabinus was besieged in it by the troops of Vitellius (4); and the third time in that dreadful conflagration which happened in the reign of Titus (5). Whether the standards were destroyed in the first of these fires is uncertain; but they could hardly escape the fury and confusion of the second, when, according to Pliny, the temple of Juno Moneta seems to have been burnt to the ground (6). And, if we may credit Xiphilin (whose account of the third is in some measure confirmed by Spartian), not only the temple of Jupiter Capitolinus, but those adjoining to it, were burnt down in the last (7).

Vespasian

(1) This appears from the inscription on Vespasian's congius, and from R. Fannius. And Jul. Capitolinus in Maximinis, mentions the amphora Capitolina.

(2) Hyginus de Limit. constituend. near the end, distinguishes the Roman foot by the name of Pes Monetalis. That this temple was in the Capitol, appears from Livy, lib. vii. c. 28. Cicero de Divin. lib. i. and Pliny, lib. xxxiv. cap. 7. Suidas (v. Μονητεῖς) tells us the mint was kept here.

(3) Taciti Hist. lib. iii. This fire is likewise mentioned by Plutarch and Appian.

(4) Tacitus, *ibid.*

(5) Xiphilin in Tito. Sueton. in Domit. c. 5.

(6) Pliny, lib. xxxiv. c. 7. says, "Ætas nostra vidit in Capitolio, priusquam id novissime conflagravit, à Vitellianis incensum, in cella Junonis canem ex ære, &c." And if the donatives were destroyed, it is not likely the standards should be saved in a scene of such confusion.

(7) Τὸν τε νεῶν τῆς Διὸς τῆς Καπιτωλίου, μετὰ τῶν συννάων αὐτῆς, κατεκαυσεν. Xiphilin in Tito. Spartian, enumerating the public

Vespasian rebuilt the Capitol after the second conflagration, and restored the antient records from copies of them that were got abroad (8), and probably the standards at the same time. The congius was restored by weight, according to the *Plebiscitum Silianum*, as the inscription on it testifies. The quadrantal was too cumbersome a vessel for common use, to which the congius (like our gallon) was well adapted; so that I do not see what other purpose it could serve, but to adjust the congius to its capacity, and the foot to its side: but here we see the congius adjusted by weight, and it is not very likely that a new quadrantal should be made for no other end but to adjust the foot by, when so many copies of the old standard were extant.

Therefore it is not improbable, that the standard of the foot was at this time restored, without any regard to its relation to the quadrantal.

But as to the difference between the foot derived from the congius, and that found from other authorities, I must farther observe, that the correct adjustment of weights to measures, is a very difficult matter, even in this age, and in this kingdom, where workmanship is arrived at a high pitch of accuracy. And what errors rude workmanship is liable to, sufficiently appears from the weights *Pætus* and *Villalpandus* have given of the congius. Therefore I see no reason to reject the testimonies of those authors,

buildings restored by Adrian, mentions most of those that *Xiphilin* says were burnt in this fire. Domitian rebuilt the Capitol. *Suet. in Domit. c. 5.*

(8) *Sueton. in Vespas. c. 8.*

who

who say that the cubic foot contained a quadrantal of wine; and as little to believe that these two standards were ever truly adjusted to each other.

But had the original standard of the Roman foot been truly adjusted to the quadrantal, and continued invariable from the time of its first establishment, yet a false measure of it might at one time or other have got into common use at Rome, as well as a false measure of the French foot did at Paris; where in the year 1668, the mason's foot was found to exceed the foot of the Chatelet by $\frac{2}{7}$ of a Paris inch (9), which is above $\frac{1}{4}$ of a London inch: and the unaccountable negligence which appears in the Roman coinage, gives sufficient ground to suspect they were not more accurate in their measures.

LXX. *A Description of a metalline Thermometer; by Keane Fitzgerald, Esq; F. R. S. Communicated by the Right Hon. George Earl of Macclesfield, President of the Royal Society.*

Read May 22, 1760. **I**T is universally allowed, that all bodies, whether solid or fluid, are expanded and contracted by heat and cold; and, as far as experiments of this kind have yet reached, it appears, that scarce any two bodies of different natures, or even of the same; are expanded, or contracted equally by the same degrees.

(9) Picard, in the paper *De Mensuris*, quoted above.

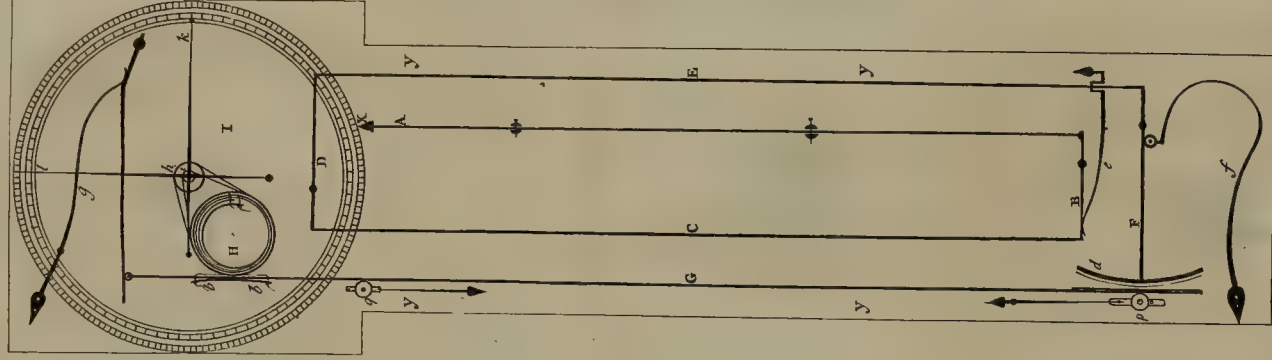
The

The advantages that may be derived from being able, by proper instruments, to ascertain the degrees of natural and artificial heat and cold, with respect to philosophical inquiries; and also to several useful, and common purposes in life; have induced many ingenious persons to apply their thoughts that way; and endeavour to form some certain standard, or gage, by which this may be obtained. The three principal bodies that have been made use of for this purpose; are air, alcohol, and mercury.

Mr. Boyle, the great promoter of experimental philosophy, made a thermometer on the principle of air, which, to a certain degree of heat or cold, answered very minutely. Alcohol, or spirit of wine, has been more generally used; but has been found to lose, in time, much of its expanding quality; and also to be frozen by an intense degree of cold. Mercury, as not deemed subject to these inconveniencies, has therefore been allowed the most proper for the purpose.

The learned Dr. Halley has observed, that mercury expands very sensibly at first, attaining very nearly the same height, some time before the water boils, in which it is placed; that it does on the strongest boiling: whereas spirit of wine expands gradually, as the heat increases; but slower at first, and faster after it is well warm; and, at a certain degree of heat, which wants much of boiling water, being almost tolerable to the touch, it boils vehemently; emitting bubbles, which, coming into the neck of the thermometer, lift the incumbent spirit to the top: from whence he concludes, that the
expansion





expansion of fluids graduated by equal parts, is not a sufficient standard of heat and cold.

Mr. Fahrenheit has since improved the mercurial thermometer to a great degree, and brought it to as much perfection as, perhaps, it will bear. He has remarked, that when the barometer shews a greater degree of pressure of the atmosphere; the same liquor will receive eight, or nine degrees of heat, more than when the barometer is at the lowest. But whether this proceeds entirely from the liquor's receiving a greater degree of heat, by the pressure of the atmosphere, may be a matter of some doubt; as it seems, by comparing the mercurial, with other thermometers; to be affected, in some measure, by the pressure of the atmosphere, in all degrees of heat and cold.

The making of metalline thermometers has been hinted at by many; particularly by Mr. Smeaton, in his curious observations on the expansion of metals, who recommends zink, or spelter, as most capable of expansion, and fittest for the purpose. I have endeavoured to make one on this principle, which I take the liberty of laying before this Society, with a description of its construction, and an account of the few observations I have been as yet able to make on it. [*Vide Tab. XVIII.*]

It is composed of four metal bars, which act by a combination of levers, and pullies. The upper end of the bar A, which is 2 feet long, is opposed by a flat piece of metal x , screwed fast to a piece of deal board; y, y, y, y , and its lower end rests on a small hemisphere, placed on the shorter arm of the lever B, which is fixed to the board y .

By

By the expansion of the bar A, the shorter arm of the lever B is pressed down: and as the space described by each of the points of a lever is as the distance from the fulcrum; so the longer arm of the lever B, which is $2\frac{1}{2}$ as long as the other; is raised proportionably; and the second bar C, 2 feet 2 inches long, the lower end of which is placed to the point of the lever B, by a small pin, round which it is moveable; and the upper end, in like manner, to the shorter arm of the second lever D; is raised accordingly; and thereby the longer arm of the lever D, which is $2\frac{3}{4}$ as long as the other, is proportionably pressed down; to which is added the expansion of the bar C, increased, in like manner, by the lever D.

The third bar E, 2 feet 4 inches long, is placed to the point of the longer arm of the lever D, and the shorter arm of the third lever F, which it presses down accordingly; and the longer arm of this lever, which is four times as long as the other, is thereby proportionably raised; to which is also added the expansion of the third bar E, increased by the lever F.

There is an arch-head *d*, fixed to the point of the longer arm of the lever F, to which the fourth bar G, 3 feet long, is fixed by two chains, which raise and lower it, as that arm rises or falls; and, at the distance of 2 feet 6 inches, there is a pulley H, 3 inches diameter. There are two cords fixed to this pulley, surrounding it different ways, one leading upwards, the other downwards, which pass through the bar G, and are fixed to two springs *b, b*, which keep them equally tight; and the bar close to the

the pulley H, which is turned different ways, as the bar G is moved upwards or downwards.

There is a pulley *p* fixed on the other side of the bar G, opposite the arch-head of the lever F, on which the bar rolls; and which, by the help of a slender spring, keeps it close to the arch-head; towards the upper end, there is another pulley *q*, on which it also moves.

The expansion of the bar G, from the place where it is fixed to the arch-head *d*, to the place where it is fixed by the strings to the pulley H, is to be added to the expansion of the other three bars, increased by the several levers.

There are two small pullies *b* and *i*, the pulley *b* one inch diameter, and the pulley *i* a quarter of an inch diameter, whose axis passes through that of the pulley *b*, like the hands of a clock; and on these are placed, in like manner, two indexes *k* and *l*, which move round the dial I, that is 12 inches diameter. These pullies are surrounded by cords, which also surround, and are fastened to springs within the pulley H. The index *l*, which may be called the minute-hand, is moved 4 times round for once the index *k* is moved round; the index *l* ranges at its extreme point 48 times as much as the bar G rises or falls; and the index *k* 12 times.

As the powers of these levers are all reversed, with respect to the pressure by the expansion of the bars; there are two counterballance springs *e* and *f*, placed under the longer arms of the levers B and D, which help to raise the bars easily; and there is another spring *g*, acting on a small roller, placed on the top of the

bar G, which returns all the bars on their contraction.

On the dial-plate I, there are three circles described. The inward circle is divided into 240° , according to Fahrenheit's scale. The middle circle is divided into 360° , which is intended to mark the expansion of different metals. The outward circle is divided into 1080 parts, to which the longer index *l* points, which marks 18 divisions for each degree of the inward circle; and 12 for each degree of the middle circle, pointed to by the index *k*.

The quarter divisions of the dial are marked I, II, III, IIII, to shew, by the index *k*, how often the index *l* has gone round.

There is a screw-hole towards the point, in the index *k*, to fix a small pencil in; which is moved by the index on a circle of wood, or ivory, and marks the highest and lowest degrees it has pointed to, for any time; which saves the continual examination requisite to form observations of this kind; and the mark of the pencil is easily rubbed out, when any new observation is intended.

The first bar A of this instrument may readily be taken out, and replaced; or any other bar of equal length and thickness applied; by which means, the instrument may be used as a pyrometer, to measure the expansion of any bar 2 feet long, with great exactness, to the 73,840th part of an inch *per* foot; and, as a thermometer, it marks the alterations in the temperature of the air, much more sensibly than any other instrument for the purpose; the range of the minute index *l* being 74 inches, by the common degrees of heat and cold of this climate.

If

If the greatest expansion, from freezing to boiling water, of the metal bars to be made use of in an instrument of this kind, is certainly known; it may be made to mark any minute degree intended. But to answer a calculation of this kind; the levers, and pullies, must be made with the greatest exactness; which I could not prevail on the workman that made mine to be so careful of as I could wish. It is easy, however, to come to a certain knowledge of the expansion, mechanically. I took a thick book, and pressing the leaves between my finger and thumb gently; measured off an inch exactly, with a pair of compasses, from the 1st page, which made 568 pages. I cut off several slips from different leaves, each of which I placed between two bits of watch-spring, which were marked, in order to place them in the same position exactly, on each trial. On examining these with the instrument, I found but few of an exact thickness; but that, on a medium, each leaf raised the minute index 130 divisions. So that the number of pages, each leaf of which is the 284th part of an inch, multiplied by the number of divisions it raises the index; shews, that each division marks the 73,840th part of an inch expansion *per* foot of the bar A, which is 2 feet long.

This bar is made of spelter, which is of so brittle a nature, as makes it difficult to file one of that length without breaking. If it is not well cast, it will be extremely hollow; as was the case of a bar I made use of, which did not expand near as much, nor as uniformly, as another of the same metal, and contracted much more readily; which I could not well account for, until it broke by accident; when

it appeared full of large cavities on the inside, though the outside seemed as fair as any other. Some of these cavities were above an inch long, and the surrounding metal, after filing, not above the thickness of a card. I would therefore recommend the weighing bars of this metal, in order to observe the difference of expansion.

The other bars of this instrument, as the founder informs me, are made of 18 parts spelter, and 2 of copper. I should imagine, that there is a greater proportion of copper than he owns, from the appearance of the metal, and the disproportion in its expansion, to what I suppose 2 parts in 20 might occasion.

Since this instrument has been made, I found, on looking over the 10th volume of Martin's Abridgment of the Philosophical Transactions; that Dr. Mortimer had, in 1735, given the Royal Society a description and drawing of an instrument he invented for the purpose; and that Mr. Johnson had also given a drawing of another, invented by Mr. Fotheringham. Although these are of a very different construction, yet, as they were formed on the same principles, I ought, in justice to these gentlemen, to mention them.

I have made what observations I could on this instrument, since the short time it is made; by comparing it with a Fahrenheit's, and a spirit thermometer; and find, that it keeps at a medium between both; not rising at first so quick as the mercury, and somewhat quicker than the spirit. On placing them together in the sun, when its heat became intense, it rose at last faster than the mercury, and not so fast

as

as the spirit ; and continued to rise for some time after the others became stationary.

I electrified the bars of the instrument, to see if the electrical fire could produce any degree of heat sufficient to expand them ; which, on the first trial, it seemed to do, by the minute index rising 6 divisions in a short time. But as I had some reason to imagine ; that this appearance proceeded rather from an increase of heat, occasioned by two gentlemen being in the room with me, when I made the experiment ; I repeated it alone the next day, leaving the door open at the time, and could not perceive the minute index to rise above one division ; which I attribute rather to the warmth that my being in the room had occasioned.

I tried the expansion of a few metal bars, from artificial freezing, with pounded ice, and water that it dissolved into ; upon which was poured half an ounce of spirit of tartar, in which Fahrenheit's thermometer descended to within one degree only of the freezing point : to boiling water, in which it rose to 211° , though the water did but scarce boil, for want of a sufficient number of lamps. The barometer stood at 30 inches, and the natural heat of the weather at 60° of Fahrenheit.

	Divisions.
* A bar of spelter 2 feet long, marked by the minute index — — — — —	1570
Spelter 18 parts, and copper 2 parts, according to the founder's account — — —	1150
Brass — — — — —	1120

* This was the bar, found, on breaking, to be hollow.

Iron

									Divisions.
Iron	—	—	—	—	—	—	—	—	785
Steel	—	—	—	—	—	—	—	—	695

Note. Each division marks the 73,840th part of an inch expansion *per* foot.

I find that these come somewhat near Mr. Smeaton's table. They are far from being as complete as I wish; but hope to be able to give a more accurate account hereafter; as it certainly requires the natural cold to be added to the artificial, in order to get at the last degree of the contraction of metals, which can be come at in this climate.

If an instrument of this kind should be deemed worthy the attention of the curious, there is no doubt, but it may be carried to a much greater degree of perfection than this, which is but a rude beginning. The friction is little or none, as the spring at the top of the fourth bar, keeps the several levers and bars in the same position on their axes, whether rising or falling. It may be made much more easily, and at a less expence, by making use of counterballance weights, instead of springs; and by a cord passing over a pulley placed on the top of the fourth bar, with a weight fixed to it, which returns the bars, on their contraction.

I have one making in this manner. The first bar of which is 3 feet long, and by a slider, which is fixed by a screw, it will measure the expansion of any bar, within that length. The powers of the levers are also greater; and the minute hand goes 12 times round, for once the other goes round; by which it marks the 300,000 part of an inch expansion *per* foot, and ranges 216 inches, by the common degrees of heat and cold in this climate.

Mercury



A Bird supposed to be bred between a Turkey and a Pheasant.

Mercury cannot be useful in trying any degrees of heat above what makes it boil: and it appears by Dr. Hinfell's account of the experiments lately made at Petersburg, that it may be frozen by extreme cold; which makes it unfit for ascertaining the extreme degrees of either.

An instrument of this kind might be made by the help of a tin casing, to come between the bars and the wood, to receive a bar of iron or steel, heated to any degree within that of its melting; without any detriment to it. And any kind of metal bar will certainly bear the most extreme degree of cold, and probably contract proportionably. I should imagine it might also be made very useful in trying minutely the expansion of different kinds of metals, fit for making compound pendulums; and the exactness of the pendulum, when made, might likewise be tried, by artificial freezing, or in hard frost, and by boiling it in water.

LXXI. *An Account of a Bird supposed to be bred between a Turkey and Pheasant; by Mr. George Edwards, F. R. S.*

To the Rev. Dr. Birch, Secretary to the Royal Society.

S I R,

Read May 22,
1760.

HAVING in my hands a bird, that, I believe, may be a curious and entertaining subject to the Royal Society, I presume, by your favour, to lay it before them [*Vide Tab. XIX.*], with its description, and what other account of it I could

could procure. I received it from the very curious and worthy Henry Seymer, Esq; of Handford, near Blandford, Dorsetshire, with his letter, dated April 9, 1760: wherein he says,—“ I have taken the first
 “ safe opportunity of sending the two birds. The
 “ large one [the subject lying before the Society], I
 “ verily believe, is an accidental cross, as we sports-
 “ men term it, between a pheasant and turkey.
 “ You are to be told, that, when the bird was just
 “ killed, the skin round the eyes was of a pale red-
 “ lead colour, and the eyes like a turkey’s. As I live
 “ near the wood where they were found, I took
 “ great pains to get another of them, but was never
 “ so lucky as to find one. There were three at first,
 “ all of which, I believe, are now destroyed. You
 “ are heartily welcome to it; but I beg the favour of a
 “ painting of it by your own hand; this I should
 “ esteem in the highest degree. I am sensible it re-
 “ quires a nice hand to copy the feathers of the back
 “ and tail; but your judgment and experience have
 “ long rendered things, extremely difficult to others,
 “ easy to you.”

In the same letter, Mr. Seymer informs me, that the bird was shot in October last, and he supposes it to be the production of the spring or summer immediately preceding, it having not, as he thinks, molted off all its first or chicken feathers, as they are called; and that it would have required a somewhat different description, had it been older. My description of it, in its present state, is as follows.

It is of a middle size between a pheasant and a turkey-hen, and shaped pretty much like a turkey; the bill, legs, and feet, are black, and shaped like a
 turkey’s;

turkey's; it hath a broad space of bare skin round the eyes, which, when the bird was living, was of a pale red-lead colour; the eyes like those of a turkey; the head, and half the neck, is covered with very short feathers, of a whitish clay colour, with transverse dusky bars, though the throat and fore-part of the neck are wholly of a light clay colour. These short feathers occupy the head and that part of the neck, which is naturally void of feathers in turkeys. On the lower part of the neck, the breast, and belly, the feathers are much longer, and of a black colour, with a purple and changeable gloss. The thighs and legs, on their fore-part, a little below the knees, are covered with feathers transversely barred with clay colour and black. The back, covert feathers of the wings and tail, are of a mixed colour, in very fine transverse lines of brown and black, though some of the coverts of the wings and tail have larger transverse bars of the abovesaid colours; the greater quills are dusky, or black, powdered with small clay-coloured spots; the inner coverts of the wings have white tips, which hide their bottoms, that are dusky. I counted sixteen feathers in the tail, the outer ones shorter by two inches than the middlemost; their colour is composed of brown and black, mixed transversely, like those on the back, though they are more dusky toward their tips; the very tips being of a bright brown: the outer borders of the side feathers of the tail are of a bay colour; the covert feathers beneath the tail are of an orange colour, crossed with black; about the vent, the feathers are white, with dusky spots. The whole upper side nearly resembles that of a hen-pheasant, but darker coloured. The feathers of the body

are all double; that is, two distinct feathers proceeding from one stem; the outer large, and of a firm texture; the inner smaller, and altogether downy.

Whether this bird be produced from a turkey-hen and a cock-pheasant, or from a turkey-cock and hen-pheasant, no one knows. I should think it rather from a hen-turkey and cock-pheasant; because their disparity in size is not near so great, as between the turkey-cock and hen-pheasant. Though the supposition, that this bird is from an egg laid by a hen-turkey trodden by a cock-pheasant, is attended with a difficulty not easily reconciled; for it is not probable, that a hen-turkey, a domestic fowl, should betake herself to the woods, and bring up her brood wild, and unobserved; which is contrary to the habit of turkeys in our country, where they are not originally natives. Why these mixed generations so rarely happen, is, I believe, because nature has fixed the inclination of every distinct species to the contrary sex of its own identical species, from which, in a wild and natural state, it will hardly ever stray. The reason of the mixtures, that we meet with, contrary to the ordinary course of generation, may proceed from some hinderance of the male's meeting with his proper female, or female with male, at the seasons, when they are by nature appointed to propagate their species, which rarely happens; for, in a wild state of nature, most animals are numerous, and, at their breeding seasons, easily meet with males or females of their own species. Disappointments of what they naturally seek, and accidental meetings of different species, near of kin to each other, cause these unnatural conjunctions, which produce uncommon

mixed species of animals. I believe, that two species widely different from each other, as water-fowl and land-birds, &c. cannot possibly conjoin, so as to produce a living mixed offspring. I have been informed, and believe it may be true, that a mixed species has been produced between our common poultry, and partridges that harbour near farm-yards.

I am, S I R,

Your most humble

and obedient servant,

London, May 22, 1760.

Geo. Edwards.

LXXII. *An Account of a late Discovery of Asbestos in France: In a Letter to the Rev. Tho. Birch, D. D. Secretary to the Royal Society, from Mr. Turberville Needham, F. R. S.*

Reverend Sir,

Read June 5, 1700. **I** Have little to add in the way of literature, except a singular discovery accidentally made, lately, in one of the French provinces, of the nature of the asbestos, or amianthus. The proprietor of a certain forge, upon taking down his furnaces to repair them, found a great quantity of this substance at the bottom. It answered effectually all the common uses of the native amianthus, either

manufactured into linen, or paper. In short, upon a progress in this inquiry, he finds, that both this, which he obtained from his forge, and the native asbestos, is nothing more, as he terms it, than calcined iron, deprived of the phlogistic; and that, by uniting the phlogistic, either with this, or the fossil amianthus, he can restore it, at any time, to its primitive state of iron. Does not this, with the discovery of lava, pumice-stones, iron in a perfect state, and, in fine, many other traces of fire observed in most of the mountains, particularly in all the great chains, and remarkably in all those under the equator, which are the highest upon the globe, seem to indicate, that the dry land, with all its eminencies, was originally raised out of the waters, by the force of subterraneous fire?

I am,

With great respect and esteem,

Reverend Sir,

Your most obedient,

humble servant,

Paris, May 17, 1760.

T. Needham.

LXXIII. *An Account of the hot Baths of Vinadio, in the Province of Coni in Piedmont; with the State of the Weather at Turin in the Year 1759: In an Extract of a Letter from Dr. Joseph Bruni, F. R. S. to Mr. Henry Baker, F. R. S.*

Dear Sir,

Turin, Feb. 24, 1760.

Read June 5, 1760. **T**HE warm baths, which have been so very serviceable to the Chevalier Offorio, run through the rocks, near the village called Vinadio, in the province of Coni.

The water is very clear, and so warm, one cannot bear the hand in it: the contents are sulphur diffused through it, and some salt almost like common salt. By evaporation, you get five, sometimes six grains, from a pound of water. I send you a small quantity of the salt, gathered from the stones by which the water runs, before the rising of the sun, for in the day-time it is not found upon them, except in winter. Where these waters run, they deposit oily particles, which, by degrees, join together, and form a soft, spongy, greenish-yellow substance, one inch almost in thickness, which is called *muffa*: this, when dry, is wrinkled, takes fire, crackles, and gives the smell of brimstone, and when entirely burnt, leaves a black ash behind. I immersed a piece of silver in these waters, which, in a few minutes, became black. Their taste is neither salt nor acid, but disagreeable. The *muffa* left for two days in common water, swells six times thicker than it was, stinks, and throws up oily particles on the surface of the water.

The

The salt does not ferment with acids. If you dissolve it in common water, and mix it with syrup of violets, it gives some appearance of a green colour : the same water poured on a solution of silver, it soon throws down a white sediment. Some say these waters contain nitre, and particles of other bodies ; but this has not been demonstrated.

The disorder of the Chevalier Offorio, for which I advised him to the use of these baths, was, that he had lost the feeling of his fingers, had a weakness in his hands, and also in his legs, insomuch, that sometimes he could not walk in a strait line, but tottered from side to side. He could not extend his toes, and the soles of his feet felt, according to his own expression, as if hard strings were drawn across them. He tried many medicines to no purpose ; but is now perfectly free from the above complaints, by the use of these warm baths ; of which he is so fully convinced, that he intends to go to them again next summer, notwithstanding at that time of year the heat is there excessive.

He bathed in these waters forty times, when the stomach was empty, in a morning ; and stayed in them, at first, half an hour, but lengthened the time, gradually, at last, to a full hour. After each bathing, he was dried with cloths, and put into a warm bed, where a plentiful sweat came on for about half an hour ; during which, the pulse beat like that in an high fever, but became quieter, as the sweating abated. When the sweating was almost over, and the pulse quite regular, he was dried again with cloths, his shirt was changed, and, sitting up in bed, he was refreshed with a glass of strong wine, and a
piece

piece of bread. After this, he rose, and dressed, and took a gentle walk.

I observed, while I was there with the Chevalier Offorio, that no patient, who came thither, received the least prejudice by the waters, though all did not receive alike benefit for their respective disorders. But one gentleman in particular, who came paralytic, in the whole inferior extremities of his body, occasioned by hard drinking, I saw so far relieved as to walk without help. I also saw others cured of disorders in the skin, and relieved in rheumatic and many other complaints.

Several persons drank the waters of a neighbouring spring, which is lukewarm, but I cannot say to any great advantage: the cures I observed were by bathing only. Where there is a partial disorder, the waters of Vinadio are pumped upon the distempered part.

Olaus Borrichius mentions a remarkable cure effected by these waters. *Vide Medic. Septentrion. Collat. tom. II. p. 855.*

I send you also an account of the weather in this country, in the last year 1759. It shews the highest and lowest state of the barometer and thermometer in every month.

The first cypher denotes the day.

Mat. signifies the morning; *D. M.* the afternoon; the following cypher the hour; and the last cypher the inches, and parts of inches, divided in 12.

In the thermometer, 0 denotes the freezing point; and the last cypher the degrees above or under the same. The thermometer is made according to that of *Monf. De Reaumur*.

B A R O-

BAROMETRO 1759.

Months.	Days.	Morn. or Aft.	Hours.	Inches.	Parts.
Gennaio	3.	D. M.	2	27	1 $\frac{1}{2}$
	17.	Mat.	7 $\frac{1}{2}$	27	10
Febbraio	25.	Mat.	7 $\frac{1}{2}$	27	1
	14.	Mat.	7 $\frac{1}{2}$	27	10
		D. M.	2	27	10
	15.	Mat.	7 $\frac{1}{2}$	27	10
Marzo	11.	Mat.	7	26	11
	15.	Mat.	7	27	6
	15.	D. M.	4	26	11
Aprile	16.	Mat.	7	26	11
	17.	Mat.	7	27	6
	4.	D. M.	3	26	11
Maggio	20.	Mat.	7	27	8
	30.	D. M.	3	27	1
Giugno	20.	Mat.	6	27	7 $\frac{1}{2}$
	4.	Mat.	6	27	0 $\frac{1}{2}$
	9.	Mat.	6	27	8
Guglio	10.	Mat.	6	27	8
	21.	D. M.	3	27	2
	5.	Mat.	6	27	7
	22.	D. M.	2 $\frac{1}{2}$	27	3
Settembre	23.	Mat.	6	27	3
		D. M.	2 $\frac{1}{2}$	27	3
	17.	Mat.	2 $\frac{1}{2}$	27	7
	18.	Mat.	6	27	7
	28.	Mat.	7	27	2
Ottobre		D. M.	2 $\frac{1}{2}$	27	2
	29.	Mat.	7	27	8 $\frac{1}{2}$
	22.	D. M.	2	26	9
Novembre	23.	Mat.	6	27	10 $\frac{1}{2}$
		D. M.	2	27	10 $\frac{1}{2}$
	3.	D. M.	2	26	11 $\frac{1}{2}$
Dicembre	24.	Mat.	7 $\frac{1}{2}$	26	11 $\frac{1}{2}$
		D. M.	2	26	11 $\frac{1}{2}$
	2.	Mat.	7 $\frac{1}{2}$	27	6

T E R M O-

TERMOMETRO 1759.

Months.	Days.	M. or A.	Hours.	Degrees.
Gennaio	{ 7. 26. 31. 12, 13, 14.	Mat. D.M.	$7\frac{1}{2}$ 2	0 6
Febbraio	{ 1. 4, 5. 17. 22.	Mat. D.M.	$7\frac{1}{2}$ 2	0 $8\frac{1}{2}$
Marzo	{ 14, 15. 28.	Mat. D.M.	7 $2\frac{1}{2}$	1 13
Aprile	{ 1. 14. 23, 24.	Mat. D.M.	7 $2\frac{1}{2}$	4 $17\frac{1}{2}$
Maggio	{ 5. 19. 13. 15, 16.	Mat. D.M.	$6\frac{1}{2}$ 3	9 21
Giugno	{ 4. 6. 19.	Mat. D.M.	6 $3\frac{1}{2}$	11 $26\frac{1}{2}$
Guglio	{ 2. 22.	Mat. D.M.	6 $3\frac{1}{2}$	$12\frac{1}{2}$ $28\frac{1}{2}$
Agosto	{ 24. 6. 18.	Mat. D.M.	6 $3\frac{1}{2}$	$12\frac{1}{2}$ 26
Settembre	{ 22, 23. 9, 10. 12.	Mat. D.M.	$6\frac{1}{2}$ 3	11 24
Ottobre	{ 6. 25, 26, 30. 1. 3. 7, 8, 9. 21. 23.	Mat. D.M.	7 $2\frac{1}{2}$	9 17
Novembre	{ 26. 19. 26. 29.	Mat. D.M.	$7\frac{1}{2}$ 2	$4\frac{1}{2}$ Sotto lo zero. 1
Dicembre	{ 28. 29.	Mat. D.M.	$7\frac{1}{2}$ 2	0 $2\frac{1}{2}$

LXXIV. *An Account of a Specimen of the Labour of a Kind of Bees, which lay up their Young in Cases of Leaves, which they bury in rotten Wood: In a Letter to Daniel Wray, Esq; F. R. S. from Sir Francis Eyles Styles, Bart. F. R. S.*

S I R,

Battersea, June 11, 1760.

Read June 12,
1760.

I Send you by the bearer a specimen of of the labour of a kind of bees, who lay up their young in cases of leaves, which they bury in rotten wood. I make no doubt but they are the same, which are described in the Transactions of the Royal Society, by Sir Edmond King, Mr. Francis Willoughby, and Dr. Lister. See Lowthorp's abridgment, vol. ii. p. 772, & seq.

Monf. Reaumur, in his History of Insects, tom. vi. p. 39. describes a kind of bees, which he calls *percebois* (wood-borers). But these, he tells us, form no cases for their young, but lay them in the holes they make in the wood, without other covering, except artificial floors, which they make of the same wood, to divide the length of the holes into separate lodgements, each of which contains a single bee. In p. 97. of the same volume, he describes another kind of bees, which he calls *coupefeuilles* (leaf-cutters); and the description, which he gives of their work, seems to answer to the specimen I send you. But he tells us, that all the specimens he had ever found, or been able to collect, of their labours, were taken out
of

of the ground ; and seems unwilling to rely on what he met with in Ray, concerning the depositing these cases of leaves in rotten willows. *Monf. Reaumur's* words are as follows.

“ *Entre les coupeuses, et même entre les coupeuses de feuilles de Rosier, il y en a, qui sçavent placer leurs étuis dans des lieux, où ils peuvent se conserver sains plus longtems, si, comme Ray le rapporte, mais ce qu’il a negligé de dire qu’il avoit vû, les étuis de feuilles, qu’il a décrits, avoient été réellement tirés de trous percés dans du Bois de Saule pourri. Pour moi je n’en ai encore vû qu’en terre, et ceux qui me sont venus de divers endroits, ont tous été tirés de terre.*”

As *Monf. Reaumur* questions the truth of the account he met with, it may, perhaps, be agreeable to the Royal Society, to have a sight of the work in question ; and, if you think so, I shall be obliged to you, if you will take the trouble of shewing it to them, at their next meeting. I should have told you, that the specimen was found in some park pales near Windsor, the latter end of last summer, by some workmen, who brought it to Mr. Lee, nursery-gardener at Hammer-smith, from whom I received it. One of the bees hatched, and crawled from his case, under my eye, on Whitsunday last ; and, by an empty case I saw, that was broke open much in the same manner, I imagine another had hatched, and flown away a little before. The remainder, I presume, will not come to life, as I observe, that some foreign insect has made its way into some of the cases ; and others may have been chilled in the winter, by the fracture of the wood, in which they were inclosed. I have

destroyed some of the cases, in examining them ; but there are enough left in their original situation, to shew how they are contrived and disposed.

I am, S I R,

Your most obedient

humble servant,

F. H. Eyles Styles.

LXXV. *An Account of a Case of a luxated Thigh Bone reduced ; by Mr. Charles Young, Surgeon, at Plymouth : Communicated by John Huxham, M. D. F. R. S.*

Read June 12,
1700.

AS John Down, a middle sized man, aged about forty, was, on the 21st August 1759, harnessing his master's horses, they suddenly took fright, and ran away with the chaise. He had his back towards the chaise, the wheel of which, as it rolled very swiftly along, struck him on the upper and hinder part of the right thigh. He fell to the ground, and was unable to rise again, and complained immediately of a violent pain in his right hip. I came to him soon after the accident, and caused him to be put to bed ; when, on examination, I found his only complaint was the violent pain about the articulation of the femur with the ischium, which was increased by any even the least motion of the limb.

limb. The toe was turned in toward the heel of the left foot, and the heel of course outward; and the whole limb, from the head of the femur to the toes end, distorted in proportion.

I thought it was very evident, at first sight, that there must be either a fracture of the femur, or a dislocation of its articulation with the ischium. The former, I think, would have been easily discoverable. But as by laying my hand on the great trochanter, while an assistant turned the foot inward and outward alternately, I could perceive, that the motion of the great trochanter corresponded exactly to the motion of the lowest end of the femur, I concluded, that, had there been a fracture, it must have been between the great trochanter and the head of the bone. And, had this been the case, I expected to have been able to discover it, by the grating, that is always to be felt, when the two broken ends of a bone are moved against one another. But no such thing being perceivable, and yet the limb so much distorted, and the pain so violent, and confined to the parts about the joint, I took it for granted, and pronounced the case to be a dislocation of the femur; and consequently endeavoured to reduce it, by the usual method of extension. To this end, two men extended the limb, by pulling on napkins tied round the ankle, while others counteracted them, by pulling on a sheet passed between his legs, and secured at the bed's head, turning the foot outward as they made the extension. This gave him great pain; but the limb soon became, in every respect, parallel to the other. It appeared as long, and, on laying it down on the bed, the great toes and heels of both
feet

feet lay exactly in the same position; and the only difference, which I could perceive in the two limbs, was, that there was a little flatness about the hip of the right side. In short, the difference between the two limbs was so little, that I began to think I had been mistaken in my opinion of a dislocation (for it was evident there was no reduction made by the extension, for that could not but have been perceived, both by the patient and myself), and that the distortion of the limb was owing to nothing else than an involuntary contraction of the muscles, occasioned by the violence of the blow. I therefore bled him, confined him to lie on his back, and charged him to move as little as possible; imagining, that rest would be his most effectual remedy. He continued in much pain for some days; but by degrees grew tolerably easy, except when the limb was moved; and at about twelve days after the accident, he could suffer the limb to be lifted to and fro gently, with little or no pain at all. Notwithstanding which, he could no more lift it of himself than at first, when it was much more painful. This embarrassed me a good deal. I was convinced there was no fracture of the limb, in any part; and I thought, from the circumstances above related, there could hardly be a dislocation. I therefore desired the opinion of two other surgeons, who, on seeing the position of the limb, and inquiring into all the circumstances, which did and had attended it, agreed with me in opinion, that it was no fracture, and that it was equally unlikely, under these circumstances, there should be any dislocation. For the right leg, when placed by the side of the other leg, as I have said already, was exactly parallel

parallel to it, and continued so, unless the patient, either when asleep or at any other time, moved his body, so as to drag his leg: in that case, the toe was always found inclining inwards, and the heel outward; but never so, but that it might be replaced, without the least difficulty or pain to him, but just so as one might have done by a limb that was paralytic. We therefore recommended rest, hoping that farther rest and time would recover the perfect ease and strength of the limb. But some days passing without any alteration, I gave him a strong purgative, and repeated it every other or every third day, for several times, in order to reduce the muscles, that I might the more plainly feel any thing through them. For though he was, in point of height, but middle sized, he was pretty fleshy, and the glutæi muscles consequently too thick to suffer any thing to be felt with any degree of distinctness through them. This answered my expectation fully; for, by repeating the purgative often, and at short intervals, his living sparingly, and being confined to his bed, he became much emaciated, insomuch, that the head of the femur was plainly felt through the muscles, dislocated backward, and lying in the space between the os ischium and os sacrum.

Of this I acquainted the gentlemen, who had examined it before, and desired them to examine it a second time; which they did, on the 15th September, and were immediately convinced, that the os femoris was dislocated, and that it was the head of the bone we felt through the glutæi muscles, in the space between the ischium and sacrum.

To

To the head of the bone's lying in this space, it was probably owing that we were at first deceived; since its being there allowed a greater latitude of motion, than could possibly have been the case any where else; which may serve to account for the parallelism of the limb to that of the other, notwithstanding a luxation.

But, on turning the foot inward and outward by turns, while an assistant laid his hand on the head of the bone, a grating was perceived, both by the patient and us all. This somewhat surprized us at first; but, as this grating was never to be perceived, without pressing pretty hardly on the head of the bone, at the time the limb was turned round, and as the head of the bone was plainly felt to turn round, whenever the thigh had that motion given to it, we concluded it could be nothing but the side of the head of the femur against the edge of the ischium.

Convinced of this, we determined to make an extension; and, to that end, brought him to the foot of the bed, and placed him on his back, with the bed's post between his thighs, which was wrapped round with cloths, to prevent its galling him. A napkin was tied round his leg, at the ankle, which two assistants pulled on, while a third turned the knee outward, and I had my hand on the head of the bone, pressing it downward. As soon as the extension began, I perceived plainly the bone sink under my hand, which I had hardly time to say, before it gave a snap, which was felt by the patient, and heard by us all, and the bone was reduced.

When he was laid on his back in the bed, the right leg appeared somewhat longer than the other; but

but that might possibly be owing to the muscles not immediately overcoming the effect of the extension sufficiently, to draw the head of the femur to the bottom of the acetabulum.

In about six or seven days he was easy, and able to walk over the room with crutches, and bear a considerable weight of his body on the right leg; and, from this time, he recovered strength very fast, and is now, and has long been, as strong in that leg and thigh, as in the other, without any even the least difference in length, or any other respect.

To Dr. Huxham.

S I R,

Plymouth, May 1760.

I Have sent you the above case of John Down; because I know it is asserted by some surgeons, and among those of the greatest character too, that a luxation of the head of the femur is little less than impossible; and that what is generally taken for a luxation of this joint, is a fracture of the bone at its neck.

A fracture of the neck of the bone probably happens more often than a dislocation. But the above case has convinced me, that it may happen, and that without any extraordinary violence, provided the force is aptly applied. Indeed any force applied in the direction of the thigh downward can hardly have any tendency to dislocate it at all; and any force from below upward will be sustained by the head of the femur bearing against the upper part of the acetabulum, till the neck, the weakest part, gives way.

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But

But though it may not be possible, that the femur should be luxated by any force applied in a direction parallel to that of the thigh, in an erect posture of the body, it is not equally impossible it should be dislocated by a force applied in a contrary direction. For, in the above case, the blow was received on the upper and hinder part of the thigh, in a direction forward, from the wheel of the chaise, which must necessarily have a tendency to drive it round forward, and consequently cant the head of the femur out of the acetabulum, backwards, where it is less deep, than it is either above or below. On the fore part of the acetabulum, it is yet more shallow, and therefore less force is required to dislocate it that way, and more especially, as there is on that side less strength of muscles to resist.

It was probably a fortunate circumstance for this man, that I thought myself mistaken in my first opinion of its being a dislocation; for, had that been clearly the case, I should, no doubt, have used every method, and every assistance to be had, in order to have reduced it immediately; and most likely, while the muscles remained in their full strength, and contracting involuntarily, and that violently too, as they will sometimes do, upon attempting an extension of them, and under which circumstance, the muscular fibres oftentimes rather break than give way, should have failed of being able to reduce it; and, in that case, the man must have remained a cripple as long as he lived: whereas now, though five-and-twenty days from the time of the accident, the muscles were so much weakened, by his being confined to his bed, and wasted by his frequently repeated purges, that

that they very easily gave way, and the reduction was effected with as little difficulty, as ever I saw in a dislocation of the humerus.

Might not the giving strong purgatives, and frequently repeating them, so as to render the muscles of strong muscular subjects more lax and weak, be a means of reducing luxations of the humerus, which are not reducible by any method of extension, which is often found the case?

I am, S I R,

Your very humble servant,

Charles Yonge.

LXXVI. *Some Observations upon a Samnite-Etruscan Coin, never before fully explained. In a Letter from the Rev. John Swinton, B. D. of Christ-Church, Oxon. F. R. S. to the Rev. Thomas Birch, D. D. Secretary to the Royal Society.*

Reverend Sir,

Read June 19, 1760. **I** Have in my small collection a silver Etruscan coin, of the size of the consular denarii, similar to one published by (1) Fulvius Urfinus, (2) Patin, (3) Vaillant, (4) Havercamp,

(1) Fulv. Urfin. in *Famil. Romanar. num. incert.*

(2) Car. Pat. *ibid.*

(3) J. Vaill. in *Famil. Romanar. num. incert.*

(4) Sig. Haverc. apud Andr. Morel. in *Thesaur. Numismat.*

and Sig. Annibale degli Abati Olivieri (5), a gentleman of Pesaro, who has written two learned dissertations upon three or four medals of the same kind; all which, with the greatest appearance of truth, he takes to have been struck on occasion of the Social war. Fulvius Ursinus, Patin, Vaillant, Morel, and Havercamp assign the medal resembling mine a place amongst the uncertain, or unknown, consular coins; though Havercamp (6), without any manner of foundation, affirms it to be a medal of Faleria. But this is not to be wondered at, as he seems to have been almost an intire stranger to the Etruscan character. Sig. Annibale degli Abati Olivieri has, with the highest degree of probability, determined it to be (7) a Samnite coin; and clearly evinced it to have first appeared in (8), or about, the year of Rome 663.

But notwithstanding this most learned and ingenious gentleman has so perfectly well explained three very rare silver Etruscan medals of C. Papius Mutilus, who commanded the Samnite troops in the Social war, that not the least doubt can be entertained of the truth of his explication; notwithstanding he has sufficiently proved the coin I am considering to have been adorned with the name of the pretor of the

(5) *Saggi di Dissertazioni Accademiche pubblicamente lette nella Nobile Accademia Etrusca dell' antichissima Città di Cortona.* Tom. II. p. 49—72. & Tom. IV. p. 133—148.

(6) Sig. Haverc. apud Andr. Morel. in *Thesaur. Numismat.* five Sig. Haverc. *Comment. in Famil. Roman. num. omn. &c.* p. 459.

(7) Annib. degli Abati Olivier. in *Saggi &c.* ubi sup.

(8) Id. *ibid.*

Marfi, who likewise distinguished himself against the Romans, in the same ruinous and destructive war; yet I cannot prevail upon myself absolutely to congratulate him, upon his discovery of all the Etruscan characters forming that name. His dissertation here does not come quite up to the point. Of this he seems himself to have been aware, when he informed the learned world, that, “ after having viewed the
 “ Etruscan inscription with a very good glass, and
 “ well considered it, he thought he could make out
 “ the following letters, P. ITTEEIV; and that if such
 “ another medal should hereafter be found, with the
 “ elements better preserved, it might perhaps con-
 “ firm his (9) conjecture.” Having therefore a similar coin now in my possession, I hope I shall be indulged the liberty of transmitting to the Royal Society a few brief and concise observations upon it; especially, as these may perhaps throw some new light upon Sig. Annibale degli Abati Olivieri's discoveries, and serve as a short supplement to his two most learned and elaborate dissertations.

The laureated head on my Etruscan denarius is adorned with an ear-ring, not visible either on Sig. Annibale degli Abati Olivieri's medal, or any of those published by Fulvius Urfinus, Dr. Patin, Dr. Vailant, and Mr. Havercamp. There is a minute difference between the lower part of the lance, held in the right hand of the galeated figure on the reverse of the piece before me, and the same part of the lance, exhibited by the other coins. The drapery likewise of the galeated figure on the first of these is

(9) *Sag. di Dissertaz. Accademich. &c.* Tom. II. p. 65, 66.

some-

something different from that of the same figure on the rest. The foot of the ox or bull seems hairy on my denarius, but not so on the others. All which singularities, together with the additional Etruscan element at the end of the inscription, representing R, and the Etruscan M on the reverse, will perhaps intitle the medal I am considering to the denomination of an inedited coin. The workmanship is good, but somewhat different from that of the consular denarii, particularly those of the same age. Mr. Havercamp justly remarks, that the legend, or inscription, here is Etruscan (10); but by no means ascertains the forms and powers of the letters, of which it is composed. He even took the Etruscan A in the exergue of his own medal, which does not appear in that of mine, for an (11) inverted R. Sig. Annibale degli Abati Olivieri calls these small silver pieces (12) Etruscan medals, because they have scarce any thing in common with those of Rome, and are adorned with Etruscan characters, or such as were in the earlier ages the letters of all Italy, then subject to the Etruscans; but at the same time he observes, that they might with greater propriety, notwithstanding the authority of the Marquis Scipio Maffei, be termed Italian coins.

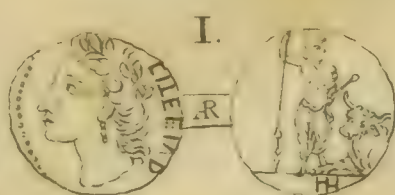
As I remember not to have seen an alphabet of the Samnite-Etruscan characters, exhibited to the public; I have taken the liberty to transmit one to the Royal Society, deduced from some very valuable Samnite-

(10) Sig. Haverc. ubi sup. p. 459.

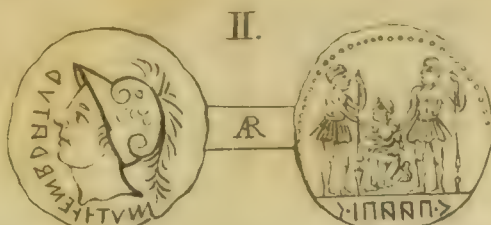
(11) Id. ibid.

(12) *Sag. di Dissertaz. Accademich. &c.* Tom. II. p. 49.





Penes Joannem Swinton, S.T.B. Oxoniens. R.S.S.



Pines Joannem Swinton, S.T.B. Oxoniens. R.S.S.



In Gaz. Pemb.

The Samnite-Fratrian Alphabet, deduced from the Coins of C. Papius Mutilus, Veturius, Capua, Acerræ, &c. and used in several parts of Italy so late as the Year of Rome 663.

A B C D E F G H I J K L M N O P Q R S T U V W X Y Z

a b c d e f g h i j k l m n o p q r s t u v w x y z

0 1 2 3 4 5 6 7 8 9

A
B
C
E
H
T H
I
L
M
N
P
R
S
T
V
P H *or* F
E I

Etruscan coins. The ducts of the letters are drawn with so much accuracy, that they may be intirely depended upon. [*Vide Tab. XX.*]

The inscription here mentioned, standing behind the laureated head on this denarius, is drawn from the right hand to the left, after the Etruscan manner, and consists of eight letters. The first of these is the *Digamma*, equivalent to V or F, as it appears on the celebrated tables of Gubbio, and several Etruscan coins. The second is apparently I, and the sixth will undoubtedly be taken for the same element. The third is manifestly T, both on my medal and every similar one hitherto published. The fourth and fifth must be E, though the latter of these has not been perfectly well preserved on my coin. The seventh will be universally considered as V, though only part of it is visible on the piece before me. The eighth is the Etruscan R, as presented to our view by one of the medals of C. Papius Mutilus, by the tables of Gubbio, and by other Etruscan remains of antiquity. It is very perfect and intire, and not exhibited by any such piece that has hitherto appeared (13). Sig. Annibale degli Abati Olivieri seems inclined to take the first element of the inscription (14) for P. But this is probably done in order to accommodate the name to the corrupted text of Velleius. The form he himself has given us of this letter bears scarce any resemblance to that of the Etruscan P, on the medals of C. Papius Mutilus; but approaches near enough to that of the

(13) *Tab. Eugubin.* pass. Vid. etiam *Gor. Mus. Etrusc.* Vol. II. & *Dempst. De Etrur. Regal.* Tom. I.

(14) *Sag. di Dissertaz. Accademich.* &c. Tom. II. p. 65.

Digamma, or Etruscan V, above-mentioned. (15) Mr. Havercamp's denarius, as well as mine, and that described by M. Morel, sets the point here insisted upon beyond dispute.

From the foregoing observations we may collect, that our inscription, in Roman, or modern Latin, letters, will be FITEEIVR, or VITEEIVR; which, in the cacography of the Etruscans, as their rude and uncouth manner of writing is termed by Sig. Passeri (16), seems to answer to the VETVRI, or rather VETVRIVS, of the Romans. If this be admitted, it will be allowed extremely probable, that the name of the general, who commanded the Marsi in the Italian war, was FITEEIVR, VITEEIVR, or VETVRIVS, and neither VETTIVS nor INSTEIVS, as Sig. Annibale degli Abati Olivieri seems inclinable (17) to believe. This general is called Vettius, or C. Vettius, by (18) Claudius Quadrigarius; Insteijs, or Insteijs Cato, by (19) Velleius; 'Ουέτιος Κάτωρ, Vetius Cato, by (20) Appian; P. Vettius, or P. Vettius Scato, by (21) Cicero; Vellius, or Vellius Cato, by (22) Florus; and T. Vettius, T. Vetius, T. Vetitius, and T. Vietius, in different manuscripts

(15) Sig. Haverc. ubi sup. p. 459.

(16) Jo. Bapt. Passeri. Pifaurenf. *De Num. Etrusc. Pæstanor. Dissertat.* p. 18. in *Symbol. Literar. opusc. var. &c.* Vol. II. Florentiæ, 1748.

(17) *Sag. di Dissertaz. Accademich. &c.* Tom. II. p. 65, 66.

(18) Claud. Quadrigar. apud Senec. *De Benef.* Lib. iii. c. 23.

(19) Vell. Paterc. Lib. ii. c. 16.

(20) Appian. Alexandrin. p. 375.

(21) Cic. *Philip.* xii. 11.

(22) Flor. apud Sig. Havercamp. in not. ad Eutrop. p. 206. Lugd. Batavor. 1729.

and

and printed editions of (23) Eutropius. From whence we may infer, that the name of this general has been corrupted in most, if not all, of the texts of those authors; even upon the supposition, that our coin bears not any relation to him. But as (24) Sig. Annibale degli Abati Olivieri has clearly evinced the truth of the contrary supposition, the variety of corruptions here produced will add no small weight to what is more than suggested by the denarius I am considering; so that the inscription exhibited by this small medal is even supported by those corruptions, and will undoubtedly, in the opinion of the most learned men, ascertain the name of the general, who commanded the Marfi in the Italian war.

In farther eviſtion of what is here advanced, it may be obſerved, that the proper name VETVRIVS itſelf has alſo been indisputably corrupted in other paſſages of antient writers. In one of Cicero (25) it has been transformed into Betucius, and in one of Plutarch (26) into ΒΕΤΕΤΙΟΣ, or Butetius. Which being maturely weighed, it will not appear ſtrange, that this general's family name ſhould have met with ſo hard a fate. For that the piece I am endeavouring to explain is one of his coins, will be acknowledged more than probable, if we conſider with proper attention what has been urged by Sig. Annibale degli Abati (27) Olivieri in favour of

(23) Eutrop. *Hiſt. Rom.* Lib. v. c. 3. p. 205, 206. Ed. Havercamp. Lugd. Bat. 1729.

(24) *Sag. di Diſſertaz. Accademich. &c.* Tom. II. p. 65, 66.

(25) Cic. in *Bruto*.

(26) Plutarch. in *Quæſt. Roman.*

(27) *Sag. di Diſſertaz. Accademich.* Tom. II. p. 65, 66.

such a notion. He has demonstrated this denarius and one of C. Papius Mutilus to be of nearly the same size and workmanship, to be adorned with inscriptions consisting of the same sort of characters, and to have the very same type on the reverse. He has also incontestably proved, from the best authorities that can be produced, that C. Papius Mutilus, mentioned by the coins he has so learnedly explained, was cotemporary with our Veturius, or, as he denominates him, P. Itteius, and the general under whose conduct the Samnites so greatly distinguished themselves, in the aforefaid ruinous war, against the Romans.

But this will still receive a farther accession of strength from another consideration, relative to the point I am now upon, which ought by no means to be omitted here. Two (28) medals of the Veturian family and one of (29) C. Papius Mutilus exhibit almost exactly the same type on the reverse, though the inscriptions they bear consist of different characters. This amounts at least to a pretty strong presumption, that Ti. Veturius, whose name occurs on one of the Roman denarii, was either the pretor of the Marfi himself or related to some commander of the allies of the same name; as (30) Mr. Havercamp takes the piece to have been struck not long after the Italians had the right of suffrage conferred upon them by the Romans, in order to perpetuate the memory of that remarkable event. I say, " or " related to some commander of the allies of the same

(28) J. Vaill. in *Num. Fam. Vetur.* p. 533.

(29) *Sag. di Dissertaz. Accademich.* &c. uti sup.

(30) Sig. Haverc. *Comment. in Famil. Roman. num. omn.* &c. p. 438, 439, 440.

" name;"

“ name;” because it cannot perhaps be so easily proved, that he was our Veturius himself. For we are told by Claudius (31) Quadrigarius, in a fragment of his preserved to us by Seneca, that one of this general’s slaves killed him, in order to prevent his falling into the hands of the Romans; which seems to intimate, that he was dead before the conclusion of the Social war, or at least before the Italians were put upon an equal foot with the Romans. However, his name might have been impressed upon this denarius, after his death. Whether or no the Veturian family itself deduced its origin from the Marfi, notwithstanding the silence of Fulvius Ursinus, Dr. Vaillant, and Mr. Havercamp, on that head, I shall not take upon me to determine.

Be this, however, as it will, from the family name **VIDEIVR**, **FITEEIVR**, or **VITEEIVR**, on the silver Etruscan, or Samnite, medal, which at present is the principal object of my attention, we may infer, that the Etruscan orthography was very rough and unpolished, at least in some of the Italian provinces, so late as the 662d, or 663d, year of Rome; when that of the Romans, now called the Latin, was arrived at almost the last degree of perfection. It must nevertheless be owned, that the later Latin, or Roman, letters (so denominated in contradistinction to the antient Latin characters, undoubtedly the same with the Etruscan, and used by the Romans themselves in the earliest times) as well as the Etruscan, were in vogue amongst several of the Italian states, when the medals of C. Papius Mutilus and Veturius were struck. This most evidently appears, from the silver

(31) Claud. Quadrigar. apud Senec. ubi sup.

pieces, with the word *ITALIA* upon them, explained by Sig. (32) Annibale degli Abati Olivieri, in his first dissertation; and from a similar one in my little cabinet, hitherto unpublished, which I lately acquired. Hence some learned men may perhaps be inclined to believe, that both the denarius of the Veturian family and the coin by me here referred to our Veturius did in reality exhibit the name of the general of the Marfi, and first appeared about the 663d year of Rome. With regard to the word *QVIEETIQ*, or *VETVRIVS*, mentioned in the beginning of this paragraph, I shall beg leave to remark, that it might as easily have been transformed into *VETTIVS*, or *VETIVS*, in the manuscripts and printed editions of some antient writers, as *PAPIVS* into *APONIVS* in those of others; and that the (33) word *APONIVS*, for *PAPIVS*, stands in the text of Diodorus Siculus, as cited by Photius. With which observations I shall conclude what I have here to say, in defence of my lection of this Etruscan inscription.

As I only intend this paper to be a short supplement to Sig. Annibale degli Abati Olivieri's two most excellent dissertations upon his Samnite coins, it would be intirely superfluous to attempt an explication of the types exhibited by those coins; especially, as this has already been done in so ample a manner, with so much sagacity and erudition, by that very ingenious gentleman himself, the learned Sig. Bianconi, and the most acute Sig. Passeri, that scarce any thing new or material on that head can be added to their observations. However, it may not be improper

(32) *Sag. di Dissertaz. Accademich. &c.* Tom. II. p. 57.

(33) *Diod. Sic. apud Photium, in Myriobibl.* p. 1185, 1186. Ed. Schot. Rothomagi, 1653.

just to remark, that both the denarii of C. Papius Mutilus and the piece by me attributed to our Veturius (34) were coined, in commemoration of the alliance entered into by the Italian states against the Romans, about the time of the commencement of the Social war; whereas the Roman medal of Ti. Veturius (35), according to Mr. Havercamp, was probably struck after the conclusion of that war, and even after the admission of the Italian allies into the city, the memory of which it was perhaps intended to perpetuate, by the Romans.

That the Etruscan pieces here considered were struck on occasion of the Italian war, or war of the allies, is likewise abundantly clear, from a most rare Etruscan medal in Lord (36) Pembroke's noble collection, never hitherto explained. It is adorned with such a laureated head as occurs on Veturius's coin, and on the reverse a bull goring with one of his horns a furious wild beast, resembling a monstrous dog, he had got under him, together with the Etruscan inscription, *VTNRDNE JITVM IONNNT >*, C. PAAPI MVTIL EMBRATVR, though very indifferently copied. This is a fourth Etruscan, or Samnite, coin of C. Papius Mutilus, the Samnite general; which Sig. Annibale degli Abati Olivieri had neither seen nor heard of, when he published those two learned dissertations, which this paper is intended to render a little more complete. The bull on Lord Pembroke's medal undoubtedly denoted the states of Italy, or rather

(34) *Sag. di Dissertaz. Accademich. &c.* Tom. II. p. 65—72. & Tom. IV. p. 140—148.

(35) Sig. Havercamp. *Comment. in Famil. Roman. num. omn. &c.* p. 438, 439, 440.

(36) *Numism. Antiqu. &c.* Thom. Pembroke, et Mont. Gomer. Com. P. 2. T. 87. num. 10.

the union of those states, combined against the Romans, as the fierce animal under him did the senate and people of Rome; the whole type pointing out to us the hoped for superiority of the former over the latter, or rather being intended as a prognostication of such superiority. That the bull was the symbol of several Italian cities, or states (37), appears from the coins of Posidonia, Thurium, Arpi, &c. and that the (38) word Ἰταλῶν, Italus, or Italian, in the old Etruscan tongue, the primitive language of all Italy, signified a bull, whence probably that animal might become the symbol of at least a very considerable part of this country, is a point so exceeding clear, that it will not admit of a dispute.

To what has been said we may add, that the Italians looked upon the Roman republic, about the time of the Social war, as a receptacle of wolves, always ready, if not determined, to ravish their liberty from them. This we may collect from the following words of Pontius Telestinus, in Velleius: (39)—*adjiciens, nunquam defuturos raptores Italicæ libertatis lupos, nisi silva, in quam refugere solerent, esset excisa.* This observation may possibly throw some new light upon Lord Pembroke's medal, as well as upon the type of my Etruscan denarius, and the similar one of C. Papius Mutilus's coin, which seems to have a little embarrassed the learned (40) author I have all along here had principally in view.

(37) Hubertus Goltzius, in *Magn. Græc. Numism.* pass.

(38) Apollodor. Lib. ii. Isacius ad Lycophronem, & Tzetzes in *Chiliadibus*.

(39) Vell. Patere. Lib. ii. c. 27.

(40) *Sag. di Dissertaz. Accademich.* &c. Tom. II. p. 66, 67. & Tom. IV. p. 141.

As the foregoing Etruscan coins are proved to have been struck about the 662d or 663d (41) year of Rome, the Etruscan alphabet then undoubtedly prevailed over a very considerable part of Italy; though the forms of some of the characters, particularly those of A, P, and I, were not precisely the same with those of the equipollent letters used in Umbria and the (42) Proper Etruria. I shall only at present beg leave farther to remark, that the reverse of my Etruscan denarius has handed down to us a new form of the Etruscan M, placed in the exergue, which has never been before observed on any of the Etruscan remains of antiquity; and that I am, with sentiments of the highest respect and esteem,

S I R,

Your most obliged,

and most obedient,

humble servant,

J. Swinton.

Christ-Church, Oxon.
May 19, 1760.

LXXVII. *De Proximo Veneris sub Sole Transitu.*

Ad Londinensem Regiam Societatem.

Read June 19, 1760. I. **N**EMO fanè ignorat Astronomus, quanti interfit ad absolutam perfectionem Astronomiæ, accurate nosse distantiam Terræ

(41) Annibal. degli Abat. Olivier. ubi sup. & alib.

(42) Gor. *Mus. Etrusc.* Annibal. degli Abat. Olivier. in *Sag. di Dissertaz. Accademich.* &c. Tom. II. & Tom. IV.

à Sole, à qua reliquorum Planetarum, Cometarumque pendent absolutæ distantiae, moles, ac massa, et alia sane multa, quæ notam exigunt parallaxim Solis cum hac ipsa distantia tam arcte connexam, ut utriuslibet investigatio pro uno eodemque haberi soleat. At hoc tantum nobilissimæ facultatis fundamentum nondum intra satis arctos limites definitum esse censetur passim vel post ipsos, utilissimos illos quidem, Caillii labores, ex quibus nescio, an omnino certum intra decimam sui partem habendum sit. Quamobrem nulla videtur prætermittenda occasio promovendæ ulterius nobilissimæ atque utilissimæ perquisitionis, quod ad eos maximè pertinet doctorum hominum cœtus, qui publica auctoritate et constituti et sustentati multo facilius, quam privati homines et inire possunt, et vero etiam debent, rationes ad rem perficiendam idoneas.

2. Una ex occasionibus rei bene gerendæ maximè opportunis habebitur sequenti anno, nimirum expectatissimus et rarissimus Veneris sub Sole transitus; quæ occasio ne sine optatissimo fructu effugiat, id vero potissimum ad Anglicanam nationem curare pertinet, et ad hanc Regiam Societatem in primis. Duplex autem videtur mihi esse ratio, cur hujus occasionis non omittendæ cura ad Angliam in primis, et ad hunc doctissimum cœtum, pertineat. Primo quidem, quod in ejus occasionis spem jam ab anno 1716, in Philosophicis Transactionibus universam primus Europam erexit ingens illud Astronomiæ lumen, ac Anglicanæ nationis decus, Halleyus, Astronomos omnes ad eam rem excitans, quem et illud maxime doluisse constat, quod nullam haberet spem vitæ usque ad id tempus producendæ, ut tanto bono frui posset. Deinde, quod nusquam alibi instrumenta ad rem bene gerendam

gerendam apta ad eum perfectionis gradum, quo hic est opus, redacta sint, ut hic in Anglia.

3. Primum caput per sese patet; quam enim dedecet illud, si, quod ne suspiciari quidem possum, ea gens tantam opportunitatem negligeret, quæ ipsum detexit prima, quæ cæteras nationes ad eam arripendam incitavit, quæ tot alia præclarissima inventa ad Astronomiam in primis perficiendam huc usque protulit! Ipsi nimirum debet Literaria Resp. generalem immortalis Newtoni gravitatem, quæ una ad Cælum Astronomis referandum censetur jam per universam Europam quædam veluti clavis certissima; ipsi Cometarum orbitas, et Lunarium aberrationum periodum ab Halleyo definitas; ipsi aberrationem luminis, et nutationem axis incredibili et patientia, et sagacitate, et vero etiam felicitate detectam à Bradleyo, à quo uno, definitis jam demum stellarum fixarum locis ad datum tempus quodcumque intra arctissimos unius, vel alterius secundi limites, Astronomia accepit illud, ut perfici demum possit; ipsi alia innumera, quæ tantam Anglicanæ nationi laudem et gloriam pepererunt, quibus omnino vel tantummodo commemorandis nullus adest finis. Quamobrem huc universa defixos habet oculos Europa, nihil dubitans, quin eadem persistet adhuc ad nobilissimam facultatem promovendam alacritas, ac vetustum omnium bonarum artium studium perseveret, et qui alios ad hanc ipsam rem bene gerendam incitarunt scriptis, exemplo quoque suo sollicituri sint.

4. Quod autem ad alterum pertinet caput, instrumenta omnino necessaria ad hujusmodi observationem sunt horologium oscillatorium et telescopiam: horum alterum hic primum satis perpolitum, et rite dispositis

tam variis delicatissimæ machinæ partibus, ac impedito caloris effectu, summam demum æquabilitatem accepit, nec usquam alibi nunc etiam majorem habet perfectionem, quam hic: alterum hinc fere tantummodo prodit satis idoneum ad subtiliores ejusmodi observationes, sive dioptricum adhibendum sit, sive catadioptricum. Illud quidem post mirum sane recentissimum Dollonii inventum ad eum perfectionis apicem redactum est, quem olim nunquam sperare licuisset: hoc autem hic primo inventum, eo Shortii in primis diligentia atque industria perduxit, ut hinc potissimum ejusmodi instrumenta præcipui omnes per Europam Astronomi sibi expetant, atque hic, quotiescumque per sumptus licet, requirant. Sed et reliqua illa, quæ usui esse possunt ad uberiores certioresque ex hoc Veneris transitu capiendos fructus hic vel inventa sunt primo, vel ad summum perfectionis gradum perducta, quadrantes nimirum, sectoresque, et micrometra, tam externa illa, quæ ad alidadæ motum pertinet, quam interna, sive ea sint filaria illa vetera, quæ ocularia appellantur, sive hæc nova, quæ objectiva dicimus, quorum absolutam perfectionem debemus itidem Dollonio, in quibus omnibus, ut omittam cæteros, tantam sibi laudem Sissonius comparavit. Ea sane omnia nusquam alibi ita ad rem idonea construuntur, ut hic. Quamobrem quis illud plane non agnoscat, eam gentem, quæ instrumenta ad utilissimas observationes instituendas præ ceteris omnibus aptissima exhibeat, decere itidem ante omnes alias iisdem uti, et Astronomos, quos habet plurimos, ad capiendos ex iisdem fructus ab omni Europa expectatos adhibere.

5. Accedit

5. Accedit æmulæ nationis conatus. Nam ante meum discessum Parisiis profectus jam fuerat unus ex Academicis Pondicherium in India, alter à Petropolitana requisitus Academia parabat se ad discedendum Tobolskam in Siberiam: eodem autem, quo discessi, die, de tertio ad caput Bonæ Spei mittendo deliberabatur, eratque alius, qui se offerret ad navigandum in Cyprum insulam, ac rem urgebat. Usque adeo ne in nobilissima facultate promovenda vinci se finet à Gallis Anglia, quæ tanto maiorem hoc potissimum tempore opportunitatem habet transvehendorum quocumque oporteat Astronomorum?

6. Hæc quidem ad Angliam privatim pertinent, illud vero ad gentes omnes, phænomeni raritas summa, quæ hanc vetat occasionem omittere. Id quidem phænomenum huc usque à nullo unquam Astronomo est observatum. Nam Horoccius partem vidit tantummodo Sole ante finem demerso, nec alios longe positos observatores habuit, cum quibus illam ipsam suæ observationis partem conferret. Idem phænomenum non nisi post annos octo recurret, tum nunquam iterum per totum sæculum, eoque amplius, habebitur. Quo pauciores sese occasiones offerunt, eo ipsas avidius arripiendas esse nemo non videt. Sed hisce, quæ ad incitamenta pertinent, omissis, ea, quæ spectant ad ipsam observandi rationem, et loca observationibus instituendis maxime idonea, paucis evolvam.

7. Omnis observatio in eo potissimum est sita, ut accurate determinetur momentum, quo nigricans Veneris discus lucidum Solis discum contingit. Horum contactuum habentur quatuor, bini in immerfione, bini in emerfione. Initium immerfionis et finis emerfionis

non ita accurate definiri possunt, cum in illo incipiat, in hoc definat excavari in Solis limbo foveola quædam, quæ, ubi satis exigua est, facile oculos vel maxime diligentes fallit. Nam centrum Veneris horis circiter 6 percurrent in Sole chordam minutorum circiter 25 distantem à centro Solis minutis circiter $9\frac{1}{2}$, existente semidiametro apparenti Solis minutorum proxime 16. Calculo instituto, invenio Venerem debere impendere secunda horaria $14\frac{1}{2}$ ad percurrendum in Sole unum secundum in directione ejus chordæ, et in ipso ingressu vel egressu ob ejusdem chordæ obliquitatem, erit tardior in ratione chordæ ad diametrum Solis, adeoque accessus per unum secundum requireret aliquanto etiam plus quam 18 secunda temporis; cumque foveola, quæ uno tantummodo secundo intra discum Solis procurrat, sub sensum cadere aut vix, aut ne vix quidem, possit, patet ne intra quartam quidem minuti partem sperari posse hujus initii et finis determinationem ex observatione immediata. Licebit illud initium quærere ex chordis segmenti obscuri determinatis per micrometrum objectivum, ubi jam ipsa foveola aliquanto major evaserit; earum enim singulæ, cum suam sagittam determinent, deficiunt simul partem disci Veneris immersam, adeoque immersionis initium: adhuc tamen satis vereor, ne ea etiam methodo citra plurium secundorum errorem satis definiri non possit illud primum immersionis initium, et ille ultimus finis emersionis.

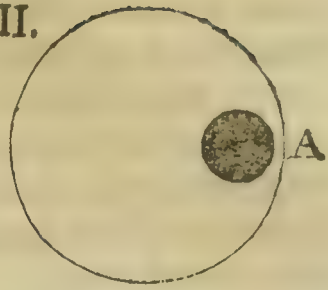
8. At finis immersionis et emersionis initium determinari omnino possunt cum evidentia summa, et perquam exactissimè. Dum enim Venus in immersione progreditur, et jam proxima est immersioni totali, habentur ut in figura I.

FIG.

FIG. I.



FIG. II.



bina in Sole veluti cornua A, et B, quæ minimo Veneris motu in fine coeunt, abeunte figura prima in secundam, in qua quoddam luminis veluti filum in A ingricantem Veneris rotundum discum cingit: contra in emerfione abrumpitur citiffime filum illud, et secunda figura abeunte in primam, bina illa veluti cornua enascuntur. Momentum, quo ipsa cornua coeunt, vel quo enascuntur, ob ipsam celeritatem, qua binæ cuspides ad se invicem vel ultimo accedunt, vel primo recedunt, definiri poterit admodum accurate.

9. Res facile patebit calculo instituto, qui est admodum elementaris. Erit enim diameter Veneris proxime unius minuti primi, et distantia cornuum, quæ est chorda communis disci Solis, et Veneris, erit in initio et fine media proportionalis inter partem diametri Veneris extantem extra Solis discum, et quadruplum residuæ partis ejusdem diametri quamproximè, neglecta nimirum curvatura Solaris disci tanto majoris respectu curvaturæ disci Veneris, cujus Solaris curvaturæ si habeatur ratio, adhuc major evadit chorda. Inde autem eruitur illud, uno secundo horario, vel binis, tribus, quatuor ante finem immerfionis, vel post emerfionis initium distantias cornuum A, B à se invicem fore proximè secundorum $3\frac{1}{2}$, 5, 6, 7, crescentes nimirum in ratione temporis subdupli-

subduplicata. Porro vel ipsa prima ex hisce quatuor distantis secundorum $3\frac{1}{2}$ selectioribus telescopiis adhibitis omnino sub sensum cadit, secunda secundorum quinque etiam communibus non prorsus ad rem ineptis, adeoque optimis telescopiis intra unum etiam secundum, communibus intra duo, ea phasis poterit determinari.

10. Quod si consideretur crassitudo fili lucidi in A in fig. II. ea singulis secundis horariis augetur per $\frac{1}{18}$ partem unius secundi, quæ ipsa in tam vivido Solari lumine utut represso à coloratis vitris, vel saltem ejus duplum, satis manifesto deprehendi debet.

11. Videtur igitur illud omnino sperari posse, ut hæc temporis momenta intra unum etiam, vel alterum secundum, absolute determinantur. Verum in re nostra nec id ipsum necessarium est nisi ad elementa Veneris accuratius determinanda. Ut eruatur id, quod pertinet ad parallaxim, et distantiam Solis considerata est sola differentia errorum, quos bini observatores committent ex radiis aberrantibus, qui inducunt procursum cornuum, et eorum distantiam minuunt, vel ex tenuitate fili lucidi non satis percellente oculi aciem. At hæc differentia et multo minor esse debet errore integro, ut patet, et in immensum imminui potest, si bini observatores telescopia adhibeant ejusdem vis diligenter inter se invicem ante collata. Ego sane nullus dubito, quin si hæc adhibeatur industria, possit errorum differentia detrudi vel infra unum secundum temporis, quam etiam sine illa omnino arbitror, modo telescopia sint satis bona, ultra duo secunda non procursum.

12. Ejus rei argumentum præter ea, quæ huc usque diximus, est mihi et illud, quod eundem

Mercurii transitum Parisiis observantes simul tres vel quatuor Astronomi, consenserint in ejusmodi phasi observanda intra quinque secunda, uti in postremo ex Parisiensis Academiae confessibus, quibus interfui, enunciatum fuit sub ipsum meum discessum. Porro primo quidem in eo transitu Mercurius aliquanto minorem potius habuit celeritatem in Sole, quam Venus sequenti anno habitura sit; deinde cum diameter apparens Mercurii circiter sextuplo minor, quam Veneris diameter, apparens sit futura, discessus cornuam à se invicem debuit esse ex eo capite minus celer in ratione 1 ad $2\frac{1}{2}$, quam in Venere futurus sit. Quamobrem hic multo minus discrimen timeri potest, quam ibi: ibi autem nec illud quinque secundorum discrimen totum huic causæ tribuendum est, sed partim etiam errorculo fortasse alicui in horologiis dirigendis, partim distantiae locorum, in quibus est observatum: unde constat, jure nos hic metum discriminis ex telescopiorum errore orti deprimere infra duo secunda horaria; licet nonnulli et decem, et viginti secundorum errorem timendum censeant, sed, uti abunde demonstravimus, omnino perperam.

13. Jam vero ut ex ejusmodi observatione optatus capiatur fructus, bina ad rem idonea, et à se satis remota, feligenda sunt loca, in quibus observationes fiant, quæ inter se comparentur, quod triplici ratione fieri potest. Prima ratio, et omnium trium maxime idonea, est ea ut observetur utroque in loco et immersio et emergio, comparando bina tempora durationum totius phænomeni; secunda, ut utrobique observetur immersio; tertia, ut emergio utrobique observetur, comparando in hisce postremis binis methodis horas locorum, in quibus observationes sunt institutæ,

institutæ, ac horam alterius ad alterius horam redu-
cendo.

14. Primam methodum proposuit jam olim Hal-
leyus, qui quidem calculo inito censuit ad rem ma-
xime idonea fore bina loca, quæ ipse selegerat, al-
terum in Septentrionali America ad Hudsoniam val-
lem, alterum in Asia Orientali circa ostia Gangis,
quibus in locis differentiam durationis totius phæno-
meni censuerat fore minutorum primorum 17, quæ
cum efficiant plusquam 1000 secunda; vel 10 se-
cundorum error è binis observationibus coalescens in
unam summam, intra centesimam sui partem defi-
niret Solis distantiam et parallaxim, quo quidem
errore ostendimus timeri vix posse errorem quintuplo
minorem.

15. Is quidem Solis parallaxim horizontalem as-
sumpserat aliquanto majorem, quam nunc censeatur,
quod illam ejus differentiam durationum auxit non-
nihil; sed id ejus methodo parum obesset. Aliud
quidpiam, quod gravioris momenti est, humani pas-
sum doctissimum sane virum affirmavit Illeus in
Parisiensis Academiæ confesso, quod paullo post etiam
typis edidit, quo quidem correcto invenitur illud, si
cum Gangis ostio conferatur locus in Septentrionali
America maxime idoneus (nam is, quem ipse Hal-
leyus ibi nominaverat, definitis aliquanto accuratius
per hosce annos elementis theoriæ Veneris, reperitur
extra eos limites, intra quos hic ejusdem Veneris
transitus apparebit) vix haberi 4 minuta ejus discri-
minis; et ejus quidem erroris se fontem etiam inve-
nisse affirmavit Illeus ejusmodi, cui summi quoque
viri sunt aliquando maxime obnoxii, subtractionem
nimirum binorum angulorum adhibitam pro addi-
tione :

tione: angulorum alter est inclinatio orbitæ Veneris ad eclipticam graduum $8^{\circ} 28'$, alter circulatio circuli latitudinis ad circulum declinationis $6^{\circ} 10'$, quos, ut Illeus affirmat, oportuit simul addere; ille autem alterum ab altero subtraxit.

16. Et mihi quidem ante hosce tres annos in ipsum discrimen inquirenti longe alia methodo, obvenerat itidem discrimen ipsum perquam exiguum. Considerabam enim illud, duplicem esse discriminis fontem, alterum ex eo, quod si seligantur bina loca hinc, et inde à polo diurnæ vertiginis, in altero, nimirum in Septentrionali America, motus diurnus debeat esse conformis motui Veneris, in altero ex parte opposita sito contrarius; unde fit, ut cæteris paribus in priore loco phænomenum diutius durare debeat, quam in posteriore, uti et si binæ naves in eandem plagam tendant, diutius in certa parte littoris ad latus siti interior exteriori apparebit, brevior vero tempore, si ferantur in partes oppositas: alterum discriminis fontem videbam esse viam ipsam, quæ intra Solem apparebit percursa à Venere. Ea enim alteri loco erit à centro remotior, adeoque brevior; alteri propior, et proinde longior. Ii bini discriminis fontes in unum additi Halleyanum mihi tempus satis proxime exhibebant. Et quidem si borealis esset Veneris latitudo, coirent utique ii bini fontes; nam chorda debita locis ultra polum borealem sitis centro propior appareret, et longior; at cum Australis ea sit, videbam sane contrarium evenire, et subtractionem binorum terminorum adhibendam, quæ mihi vix tria vel quatuor minuta relinqueret. Re aliquandiu perpensa, cum aliis tum quidem longè gravioribus curis distinerer, metuerem autem, ne mihi potius admodum

distraeto, quam tanto et Astronomo et Geometra error aliquis subrepisset; meditationem omnem abrupti, quam Isleus per otium persecutus rem omnem calculo pluribus vicibus repetito, et constructione adhibita, evoluit: et quidem est ex ejus discipulis, qui Parisiis imprefferit, eum ipsum à se de errore invento admonitum diu obstitisse, et calculorum iterationem præcepisse, donec et ipse rem diligenter exquirens idem demumprehenderit.

17. Verum Isleus ipse non errorem tantummodo invenit, sed uti diximus, et erroris fontem aperuit, et longe molestius, at simul etiam longe utilius opus aggressus, mappam binis hemisphæriis constantem delineavit, alteri similem, quam pro Mercurii transitu delineaverat olim, cujus exemplar, cum brevi disertatiuncula impressa, huc ad hanc ipsam Regiam Societatem transmissit. In ea diversis coloribus distinxit quatuor totius superficiei terrestris partes, in quibus vel ingressus tantummodo Veneris in Solem apparebit, vel tantum egressus, vel uterque, vel neuter: adjecit autem et lineas colore rubro distinctas, quarum singulæ exprimant loca omnia, quibus vel ingressus accidet, vel egressus eodem aliquo momento temporis, quod Parisiensis meridiani horis designavit.

18. Is quidem methodum, qua ad ejusmodi delineationem est usus, nequaquam exposuerat ante meum discessum Parisiis, sed pluribus id ipsum rationibus institui potest, nec theoria est sane difficilis, sed praxis admodum laboriosa, et longa. Dom. De la Lande unam è methodis ad rem idoneis proposuit in postremo ex Academiae conventibus, quibus interfui; quam dum is in schemate demonstraret, ego alio evocatus satis precipere non potui: at rem ipsam
meditatus

meditatus invenio illud, singulas ejusmodi lineas in superficie Terræ, esse totidem intersectiones superficiei conï recti cujusdam, qui facile definitur, cum ipsa Terræ superficie. Tres autem diversi conï adhiberi possunt, prout quærantur loca, in quibus simul videri debeat immersionis initium, vel emersionis finis, immersio, vel emersio centri, immersionis finis, vel emersionis initium. In primo casu angulus lateris conï cum axe est summa semidiametri apparentis Solis visi è Venere, et Veneris visæ è Sole, in secundo sola semidiameter Solis, in tertio earum semidiametrorum differentia; vertex autem omnium est in recta jungente centra Solis et Veneris, sed in secundo casu in ipso Veneris centro, in primo citra centrum Veneris Solem versus, in tertio ultra ipsum Veneris centrum ita, ut ejus distantia ab ipso centro Veneris ad distantiam à centro Solis sit in utroque ex hisce postremis casibus in ratione semidiametri veræ Veneris ad semidiametrum veram Solis.

19. Ex hoc theoremate, data Veneris positione ad datum tempus, accuratissime ea omnia definiri possunt; sed ad ejusmodi mappam delineandam, quæ rei summam quandam unico obtutu conspiciendam exhibeat, satis est unum ex ejusmodi conis considerare; quin immo satis est ejus superficiei partem admodum exiguam in superficiem Terræ incurrentem, et parum admodum inclinatam ad discum è Sole visum assumere pro plano perpendiculari ipsi disco, cujus intersectio cum hemisphærio superficiei terrestris assumptæ pro sphærica obverso Soli erit semicirculus ipsi disco perpendicularis, et sectio cum ipso disco linea recta. Circularis enim sectio ejus conï cum plano disci Terræ comparata est multo major ipso disco. Facile invenio

illud: si ab angulo supra definito lateris conï cum axe auferatur semidiameter apparens Solis visi è Terra, residuum fore semidiametrum ejus circuli visam è Sole, quæ proinde erit ad semidiametrum Terræ, ut est numerus ex illa subtractione proveniens ad parallaxim horizontalem Solis, quæ ratio evadet circiter 40 ad 1, si ea parallaxis assumatur circiter secundorum 10.

20. Inde autem fit, ut delineatis in terrestri disco meridianorum et parallelorum projectionibus ea ratione, quæ in Lunæ eclipsibus adhiberi solet, admodum facile determinantur loca omnia ad singulas ejusmodi lineas pertinentia; et multo facilius in globo res perfici possit, invento prius pro singulis semicirculis et polo eorum, et distantia à polo ipso, quo pacto unico circini ductu res omnis perficitur. At pro locis, in quibus ingressus Veneris vel egressus habetur, Sole in horizonte sito, satis erit assumere circulos Terræ maximos, qui discum terminant è Sole visum momento temporis, quo è centro Terræ loca ingressus centri, vel egressus videretur, quæ momenta paucis tantummodo minutis differunt ab iis, quibus alia Terræ loca ingressum vel egressum videre debent, adeoque illi circuli rem satis pro hoc constructionum genere accuratè determinant. Idem autem Telluris superficiem in illas quatuor partes dividunt, quas supra exposuimus, quæ nimirum videre possunt vel alterum, vel utrumque, vel neutrum ex iis phænomenis. Sed hæc omnia fusiore geometrica illustratione indigerent, ubi et operationum compendia sanè multa haberi facile possunt.

21. Isleus quidem nec elementa motuum Veneris, quibus est usus, adhuc, quod ego sciam, uspiam exposuit;

exposuit; sed illud pro certo habeo, tabulas ab ipso esse adhibitas optimæ notæ, quas adhuc per selectas recentiores observationes correxerit. Ipsa quidem elementa, et potissimum hujusce nodi positio non ita certa sunt adhuc, ut omnimodus expectari debeat totalis hujus ipsius delineationis consensus cum observatione ineunda, posito etiam, quod horizontalis parallaxis Solis sit ea ipsa, quam Isleus assumpsit, sed ita parum à vero aberrare potest, ut, quod pertinet ad delectum locorum instituendis observationibus idoneorum, et fructum, qui ex iis sperari possit, quæcumque è tribus supra propositis comparisonum rationibus adhibeatur, tuto illi fidere liceat. Ad parallaxim ipsam accuratè determinandam oportebit ex ipsis observationibus hujusce transitus corrigere elementa omnia motuum Veneris circa hunc nodum, et inde accurato calculo instituto pro iis ipsis locis, in quibus observatum fuerit, inquirere differentiam temporum iis debitam in hypothese parallaxeos solaris ad calculum ineundum adhibitæ, quæ ad inventam per observationes erit quamproximè in ratione reciproca differentię temporis calculo erutæ ad differentiam observationibus definitam, siquidem discrimen earum differentiarum exiguum fuerit, uti est admodum probabile; secus vero ope parallaxeos primo correctæ restituto calculo, brevi poterit per seriem admodum convergentem ad verum ipsius parallaxeos valorem deveniri.

22. Unum illud ad Islei mappam pertinens hic omnino addendum est, ejus impressionem binis fieri cupris, quorum altero Terræ loca nigro colore exprimantur, altero lineæ ad Veneris transitum pertinentes colore rubro. In pluribus ex exemplaribus impressis
non

non ita diligenter secundo cupro idem chartæ folium est impositum, ut rite omnia congruant, quod quidem ipsi exemplari contigit ad Regiam Societatem transmissio, in quo idcirco nonnullis est visus ipsius delineationis error graduum 10. At signa quædam adsunt in ipsis exemplaribus, per quæ ipse error deprehendi, et corrigi utcumque possit, et in exemplari, quod ego ad Societatis præsidem detuli, ita ambæ impressiones congruunt, ut vix ullus ejusmodi error supersit, nec is generalem rei ideam, quam hic quærimus, quidquam perturbat.

23. Ut vero ex hac ipsa mappa jam inquiramus in loca tribus illis comparationum methodis idonea, illud occurrit in primis admodum facile percipiendum, primam methodum, quæ adhibet durationem totius phænomeni, reliquis binis esse multo aptiorem. Nam in ea multo minus accurate necessarium est nosse binorum locorum longitudes. Sub eodem Capricorni Tropico à longitudine graduum 75 ad longitudinem 130 ubique totius phænomeni duratio ita evadit horarum 6, ut non nisi aliquo secundorum numero inde discedat. Quamobrem error satis ingens in longitudine locorum determinanda nihil ad sensum perquisitionem perturbat. Secus evenit in reliquis binis, in quibus comparantur inter se bina tempora immersionum, vel emersionum, in quibus ubi alterum ad alterum reducit, ad habendam differentiam temporis, quidquid erratur in definienda differentia longitudinum binorum locorum, erratur itidem in illa quæsita differentia temporum, per quam deinde Solis parallaxis est definienda. Porro longitudinum differentia multo difficilius accuratè definitur, quanquam illud commode accidit, quod ubi
semel

semel satis accurate instituta fuerit in binis locis observatio immersionis, vel emersionis Veneris, differentia longitudinum poterit deinde per observationes, quotiescunque libuerit, repetitas investigari.

24. Ut autem ex ipsa mappa definiantur loca ad hanc observationem maxime idonea, pro prima methodo seligenda sunt ea loca, in quibus duratio phaenomeni est maxima, et minima; pro secunda, et tertia seligenda sunt loca, in quibus immersio, vel emersio accidet celerrime, et tardissimè; verum ejusmodi loca sunt seligenda, ad quæ accedi possit, ut in iis satis commode liceat instituere observationem.

25. Durationem omnium brevissimam invenio prope Tobolskam in Siberia, ubi, neglectis secundis, est proxime horarum 5, 58'; est autem Petropoli, et Pekini 5^h 59'. Berolini 6^h 0'. Longissimam omnium circa australem incognitam plagam in longitudine 110° circiter, ac latitudine 60°, ubi est proxime 6^h 13'. Si eo adire liceret, haberetur discrimen cum Tobolska minutorum ferè 15; sed ea regio adiri utique non potest. Sunt loca in Nova Hollandia ad maris littus, ubi duratio est 6^h 6', quod adhuc exhibet cum Tobolska differentiam minutorum 8, sed nulla est spes eo appellendi. E locis, quæ spem aliquanto majorem appulsus exhibeant, Batavia præbet durationem 6^h 4', quod pro differentia exhibet cum Tobolska minuta 6, et 5 cum Petropoli, et Pekino, quibus in locis observationes exactæ intra duo, vel tria secunda adhuc exhiberent distantiam Solis, et parallaxim intra centesimam sui partem accuratas.

26. Immersio omnium citissimè videri incipiet circa Tropicum Capricorni in longitudine 235° in Mari Pacifico hora 2 40', omnium autem locorum maxime

maxime fero prope Cyprum insulam hora 2 57', quæ duo loca differentiam exhiberent minutorum 17. In ea error decem etiam secundorum adhuc relinqueret Solis parallaxim et distantiam definitas intra centesimam sui partem. Et in Cypro quidem observatio admodum facile institui potest, ac erat, uti monui, in Gallia, qui profectiorem meditaretur; verum plura alia ex eadem parte sunt loca fere æquè apta, in quibus observationes non deerunt. Adero ego Constantinopoli, ubi habetur hora 6 56'. Habentur Berolini minuta plusquam 55, Stokholmia, et Petropoli fere accuratè is numerus, Tobolskæ 54½, in quibus locis omnibus observatores utique habebuntur. At alter locus Mari Pacifico situs non nisi insulas habet omnino desertas longissimè à Continenti Americae sitas ad distantiam graduum plusquam 60. Is, qui aliquanto facilius id commodi Astronomiae procurare posset, est, ut arbitror, solus Hispaniae Rex, qui quidem Ulloam Astronomum egregium, et Quitensibus observationibus celeberrimum, nunc habet in Peruvia, unde in aliquam ex iis insulis navigare posset, et accuratam observationem instituere, sine qua quidquid Petropoli, Stokholmia, Berolini, Constantinopoli, in Cypro insulo fiet, erit omnino irritum, et sine ejusmodi observatione nihil omnino deduci poterit secunda hac methodo, ut prima etiam methodus sine Batavica observatione est ferme prorsus inutilis.

27. Relinquitur methodus tertia, quæ ad emergence pertinet. Loca, in quibus ea primo apparebit, sunt paullo ultra Kamtchkatkam ad extremos Orientalis Asiae fines, ubi id phaenomenum accidet hora 8^h 49'; loca, in quibus postremo videri desinet, sunt in ipsa incognita Australi plaga circa primi Meridiani latitudinem

dinem graduum 45, ubi definet hora 9 6', differentia existente iterum 17 minutorum, ut in methodo secunda. Sed licet eo deveniri non possit, adest insula S. Helenæ aptissima sane, in qua id accidit fere 9^h 4 $\frac{1}{2}$ ', et linea, quæ exhibet 9^h 4' transit prope Fernambucum in Orientali Brasiliæ ora situm, fere pervadit extremam ultimæ meridionalis Africæ oram, ac transit per ipsum caput Bonæ Spei, quæ idcirco loca omnia, si cum Kamchkatka conferantur, exhibent plus, quam 14 minuta discriminis. Verum cum habeatur finis Pekini hora fere 8 51', Tobolskæ 52 $\frac{1}{2}$ ', Petropoli 54', Berolini 55', Londini 56', Constantinopoli 56 $\frac{1}{2}$ '; observatio habita in insula S. Helenæ cum hisce locis collata exhibet differentiam majorem minutis 13, 12, 10, 9, 8 $\frac{1}{2}$, 8, quarum priores ad rem maximè idoneæ sunt, penultima etiam admodum opportuna, si illud consideretur, plures Londini observationes futuras, et quidem omnino accuratissimas.

28. Triplex autem generatim haberi potest errorum fons; primus in dirigendo horologio, vel horologii ipsius æquabilitate; secundus in determinando momento immerfionis, vel emerfionis; tertius in determinanda positione loci, in quo observatio ipsa instituitur. Ex primo capite non video, quid timeri possit, cum intra eundem diem communia etiam horologia oscillatoria, si bona sint, æquabilitatem retinere soleant potissimum circa Solstitia, circa quæ minus varia Cœli constitutio esse solet, et per altitudines correspondentes Solis et fixarum repetitas horologium satis facile ita derigatur, ut ne unius quidem secundi error inde timeri possit. Saltem adhibitis horologiis accuratissimis, et peculiari

diligentia in observando, omnis error inde oriundus evitari commode potest.

29. Ex secundo capite juxta ea, quæ initio diximus, major quam duorum secundorum error meo quidem judicio in singulis observationibus timeri omnino non potest; quin immo infra unicum etiam secundum deprimi potest adhibitis telescopiis ejusdem vis. Quamobrem, si hæc adhibeatur diligentia, in casu maximè infausto, in quo bini errores se mutuo nequaquam corrigunt, nonnisi duorum secundorum error timeri potest.

30. Et hi quidem soli sunt errores, qui in prima methodo timeri possint, licet duplicandi ibi sint idcirco, quod et immersionis et emersionis observationem ea methodus requirat, at pro reliquis videndum præterea, quid timeri possit, in determinanda positione locorum, in quibus observationes instituuntur. Determinatio latitudinis loci nec requiritur admodum accurata, et admodum facile accuratissima obtinetur per culminantium fixarum altitudines, potissimum si aliquanto majoris radii quadrantes adhibeantur. Longitudinum determinatio est aliquanto difficilior. Eam per eclipses satellitum Jovis, non credo, obtineri posse sine periculo erroris plurium secundorum; adhuc tamen, si telescopia adhibeantur ejusdem vis, et plures ejusmodi observationes instituuntur, credo utique intra sex horaria secunda posse etiam per ipsas differentiam longitudinum definiri. Verum si occurrant fixarum occultationes per Lunam, vel assumantur per egregium objectivum micrometrum plures distantiae Lunæ, à fixis cognitis, iisdem autem diebus accurate alicubi, ut Londini, et Parisiis, observetur et transitus Lunæ per meridianum, et diameter

meter apparens Lunæ, à qua pendet parallaxis quoque, credo utique, intra quatuor etiam horaria secunda, ingenti habito observationum numero, rem posse concludi.

31. Hinc eruitur pro primo methodo sperari posse determinationem intra quatuor, vel etiam duo secunda, pro reliquis binis intra sex, vel quinque, atque id etiam, si, quod fere nunquam evenit, errores omnes in unam summam coalescant. Quare si in prima methodo habeatur observatio instituta Tobolskæ, et in Nova Hollandia, ubi vidimus differentiam expectari minutorum 8, sperari potest determinatio parallaxeos intra centesimam vigesimam, vel ducentiesimam quadragesimam partem totius; si autem habeatur observatio Tobolskæ, et Bataviæ, ubi differentia expectatur minutorum sex, posse sperari determinationem intra nonagesimam, vel centesimam octogesimam partem totius.

32. Pro secunda methodo, si habeatur observatio illa in insulis Maris Pacifici circa loca, quæ supra vidimus, licet, ut recedatur à limitibus, quibus exiguo calculi errore loco motis, frustrari potest observatorem sua spes, amittatur unum minutum in tempore, ac ea observatio conferatur cum observationibus ibidem nominatis, Cypria, Constantinopolitana, Berolinensi, Petropolitana, Stokholmienfi, Tobolskæana, ubi differentia haberi debet minutorum 16, 15, 14, $13\frac{1}{2}$, admiffa etiam errorum summa secundorum 6, haberi potest determinatio intra partem centesimam sexagesimam, vel centesimam quinquagesimam, quadragesimam, trigesimam quintam totius.

33. Pro tertia methodo idem fere fructus sperari potest, si observatio instituat in insula S. Helenæ,

vel Fernambuci in Brasilia, vel Occidentalem Africæ oram, uti ex iis, quæ supra diximus patet. Observatio autem habita in insula S. Helenæ, et comparata cum Londinensi, et Constantinopolitana exhibentibus differentiam minutorum $8\frac{1}{2}$, et 8, promittunt determinationem intra octogessimam quintam, et octogessimam partem totius.

34. Binæ quidem tantummodo observationes accuratæ, et institutæ in locis fatis idoneis, rem omnino conficiunt, at plura sunt, quæ jubeant plures hac illac observatores dimittere; primo enim collatis pluribus observationibus inter se, apparebit, intra quos limites certi esse possimus de parallaxi Solis eruta, videndo, quantum ejusmodi determinationes à se invicem distent, ac assumendo medium certiores erimus, et vero valori propriores: tanti utem momenti est id, quod hic quæritur, ut nihil, quod ad majorem accurationem, et certitudinem conferre possit, omitti debeat. Deinde cavendum, ne nubes aspectum præripiant, quod quidem nequaquam accidet, si multi observatores in locis fuerint admodum diversis. Et hinc ego quidem cum in meo poemate de eclipsibus agerem ante hos octo annos de hoc ipso Veneris sub Sole transitu, et Halleyanam respicerem methodum, illud adjeci.

Credo equidem densæ circum se quaque cohortes
Effundent latè terris, quaque horrida tristi
Squalet humus glacie, quaque ustis ardet arenis,
Nec tantas imo nubes alit Æolus antro,
Ventorumque ciet gens improba, possit ut omnes
Obruere aërios tractus, atque omnibus axem
Ætherium velare locis, et condere Divam.

Fors et ego Irroquiûm sedes, et inhospita regna,
 Si tantum fortuna finet, si vita superstes,
 (Quid namque hæ vires, quid non hæc spondeat ætas,
 Dum vix ter quartus ter denis additur annis?)
 Transcurram procul et doctis comes ibo catervis.
 Spero equidem, nec me pontus turgentibus undis
 Audentem terret, glaciesque nivesque perennes.

35. Et ego quidem commutata nunc observatio-
 num ineundarum ratione non in Americam Septen-
 trionalem, sed Constantinopolim abibo, ubi observa-
 tionem institutam cum iis instrumentis, quæ mihi
 præsto erunt, non quidem optima, quæ maxime cu-
 perem, sed ad rem utcumque perficiendam idonea:
 labori certè et diligentiae non parcam. Et ea qui-
 dem observatio maximum secunda methodo habebit
 usum, si habeatur observator aliquis in iis, quas in-
 nuimus, Maris Pacifici insulis; aliquem autem adhuc
 non contemnendum in tertia methodo, si habeatur
 observator aliquis in insula S. Helenæ, vel in ora Oc-
 cidentali Meridionalis Africæ partis, vel Fernambuci
 in Brasilia.

36. Quid autem Anglicana natio, et Regia potis-
 simum Londinensis Societas, præstare possit, satis con-
 stat ex iis, quæ diximus. Maxime omnium videtur
 desideranda expeditio in insulam S. Helenæ, ex qua
 habebitur optatus fructus tertia methodo. Verum si
 æmulæ nationi Gallicæ nolit palmam concedere in no-
 bilissima facultate promovenda, opus est plures obser-
 vatores dimittat in plures plagas, et ad alias observa-
 tiones obtinendas alibi sua officia interponat.

37. Pro ipsa tertia methodo potest utique curare
 bina observationum binaria facienda cum instrumentis
 egregiis,

egregiis, et inter se collatis. Ex altera parte præter observationem in insula S. Helenæ, dimittendus esset aliquis alius vel illam Africæ oram maritimam, vel cum bona Lusitani Regis gratia Fernambucum: ex altera parte habebit omnino Societas observationes suas Londinenses.

38. Quoniam tanti à secunda, et prima methodo fructus jure sperantur, posset utique Regalis Societas ad Legatum Anglicum Matritensem literas dare, ut Regem per se etiam amantissimum et bonarum artium et scientiarum omnium, suo nomine impelleret ad præcipiendam Ulloæ observationem illam in Mari Pacifico, destinata ad illum maturè transvehendum navi, quod quidem aulæ Hispanicæ esset admodum facile, Literariæ autem Reip. exoptatissimum. Posset et ab Hollandis observationem expetere, vel in Nova Hollandia, vel Bataviæ peragendam, vel etiam aliquem è suis eo destinare, petita ab Hollandis facultate. Eo pacto Gallos in hanc occasionem intentos vinceret Anglia, ac ipsi Literarius orbis deberet exoptatissimam determinationem omnibus tribus methodis factam. Pudebit sane suorum avorum nepotes venturo sæculo, si tantam et adeo raram opportunitatem neglectam viderint, et cum ingenti Astronomiæ damno prætermissam eo potissimum tempore, quo maritimi successus maritimas expeditiones etiam literarias usque adeo faciliores reddiderant, et omnino exposcebant.

In signum animi addictissimi

et obsequentissimi,

Rogerus Joseph Boscovich, S. I.

LXXVIII. *A Proposal for discovering the Annual Parallax of Sirius; by the Rev. Nevil Maskelyne, A. M. Fellow of Trinity College, Cambridge, and F. R. S.*

Read June 26, 1760. **T**HE Royal Society having come to a resolution to send persons of ability to proper places, in order to observe the approaching passage of Venus over the Sun, the 6th of June next year; (which phænomenon was first proposed to this Society, by the late excellent Dr. Halley, forty-four years ago, as a proper means of determining the Sun's parallax, to a great degree of exactness) I take this opportunity of recommending to the consideration of this learned body another very important object in astronomy, which, I apprehend, may be cleared up at the same time, by the astronomers sent to one of those places, which will probably be judged convenient for the observation of Venus's transit; I mean the island of St. Helena.

The astronomical object, in favour of which I desire to engage your attention, is no less than the determination of the annual parallax of the Orbis Magnus; the finding out of which, from observation, would be the fullest and directest proof of the Copernican system, as the want of this proof, hitherto, has been the strongest argument made use of by those, who have withheld their assent to an hypothesis, which so fully, and yet simply, satisfies all the other phænomena.

No

No one, indeed, at this time of day, will venture to assert, that, even if no annual parallax could be found, after the greatest exertion of human art and industry, the Copernican system was not, therefore, true; since the quantity of this parallax may be so small, as to escape the reach of our sight, though assisted to the utmost. But though the defect of it would be no just argument against the Copernican system, yet the actual demonstration of it, from observation, would be a direct and convincing proof of the truth of that system.

It remains then to be considered, what hope there is now left, after astronomy has been brought to such a great degree of perfection, of being able to find out an annual parallax in any of the fixed stars.

I am sensible I may here seem to be presumptuous, in venturing to treat upon this subject, after the many accurate observations made by the learned Dr. Bradley, with an instrument constructed for this very purpose. No one can have a higher veneration than I have for the discernment and exactness of this illustrious astronomer, whose merit I have the greater opportunity of being acquainted with, by having the honour of calling him my friend. I would just beg leave to take notice, that the stars, which this astronomer observed, were such only as lay within a few degrees of his zenith: and though his observations do not seem to shew a sensible parallax in any of them, yet we cannot thence absolutely conclude, that, amongst the great number of visible stars, there are none in which it may be perceptible, till they have all of them, especially those of the greatest lustre, been observed in proper places, near the zenith, with the
like

like care and accuracy, which he has used: for, as Dr. Bradley has himself remarked, where any stars are remote from the zenith, the uncertainties of refraction, and the irregular motions of the air, become so great, as to take away from us all hopes of observing them to an equal degree of exactness.

The particular star, which, I propose, should be carefully observed, with a view of discovering its annual parallax, if sensible, is Sirius, the brightest of all the stars in the firmament, and which is therefore, probably, the nearest to us of them all. With us, this star passes the meridian, at the altitude of 22° , where the refractions are too irregular to admit of our discovering a very minute quantity, by observation: but, at the island of St. Helena, Sirius passes only half a degree south of the zenith; and, on this account, I have, for some time, looked upon that as the most proper place to make observations at, for this purpose.

I shall now beg leave, with great deference, to lay before you a particular argument, which has, for these two years, induced me to think it probable, that the annual parallax of Sirius is not so small, as to elude the nice discernment of our modern astronomers; and shall be glad to be determined by your opinion, whether it affords a sufficient presumption to us, to undertake a careful and assiduous series of observations of the distances of Sirius, from the zenith of the island of St. Helena. This argument is drawn from an examination, which I have made, of the observations of the zenith distances of Sirius, taken at the cape of Good Hope, in the years 1751 and 1752, by that excellent astronomer, and diligent

observer, the Abbé de la Caille, recited in his *Fundamenta Astronomiæ*, printed about two years since, wherein also are contained all the observations, which he has taken of the principal fixed stars; a work truly worthy of its name! Every particular observation of the same star is reduced to one epoch, that of the beginning of the year 1750, by applying the equations of aberration, precession, and deviation, to the observed places; so that the places corrected ought all to agree together, if the observations were perfectly exact, and the star was affected with no sensible motion, that was unaccounted for. Being satisfied with the excellence of the observations, I was tempted to examine those of some of the principal fixed stars, in hopes of discovering some sensible differences in the observations made at different times of the year, when a parallax, if there had been any, would have had the greatest effect. But I found very few stars, the observations of which were sufficient in number, or taken at proper seasons of the year, to give room for any inference at all. Fortunately, however, the observations of the zenith distances of Sirius were more in number, and, what is still of more consequence, in this case, made in various, and some in opposite seasons of the year: and, upon looking them over, I was agreeably surprized to find a very sensible difference in the observations made at different times, agreeing in direction with what a parallax ought to produce, the zenith distance of Sirius in July coming out no less than 8" greater than in the opposite season of the year, namely in December and January: the zenith distance also in March and April

April being of an intermediate quantity, as it ought to be: for, Sirius being in conjunction with the Sun in June and July, it is evident he must be then farther from the Earth than in December and January, when he is in opposition to the Sun, and, consequently, his latitude and declination, which are both south, must be less in the former case, than in the latter; and therefore, as he passes north of the zenith at the cape of Good Hope, his zenith distance must be greater in the former season, than in the latter, as the observations indicate: but, in March and April, when Sirius is in quadrature with the Sun, and equally distant from the Sun and Earth, his apparent latitude and zenith distance must be the same as the true, or that which would obtain, if the Earth was translated to the Sun, and consequently, a mean between the zenith distances in July and December, agreeably to the observations.

I shall now lay before you the observations themselves, together with the calculations, which I have made of the values of the parallax for each, the maximum being assumed $9''$, which I find will best reconcile the observations with one another.

		Zenith distance of Sirius at Cape, observed and re- duced.		Annual parallax to reduce observat. to mean.	Zenith distance corrected for an- nual parallax.		Difference between each observation cor- rected, and the mean of the distances cor- rected.
		°	'	"			
1751.	July 12.	17	31	19.1	—	4.3	17 31 14.8 — 1.1
	July 13.	17	31	19.3	—	4.3	17 31 15.0 — 0.9
	July 14.			21.7	—	4.3	17.4 + 1.5
	July 15.			19.7	—	4.3	15.4 — 0.5
	July 16.			19.0	—	4.3	14.7 — 1.2
1752.	April 5.			16.7	—	1.0	15.7 — 0.2
	April 3.			15.7	—	0.8	14.9 — 1.0
	March 28.			14.0	—	0.4	13.6 — 2.3
	March 14.			13.1	+	0.7	13.8 — 2.1
	March 8.			15.2	+	1.2	16.4 + 0.5
	March 5.			18.8	+	1.4	20.2 + 4.3
	March 4.			17.3	+	1.5	18.8 + 2.9
	March 1.			15.9	+	1.6	17.5 + 1.6
	Feb. 17.			12.8	+	2.6	15.4 — 0.5
	Jan. 21.			11.1	+	4.0	15.1 — 0.8
1751.	Dec. 31.			11.6	+	4.5	16.1 + 0.2

Mean of zenith distances corrected }
for parallax - - - - - } 17 31 15.9

The first column shews the year and day of the observations; the second the zenith distances of Sirius at the cape of Good Hope, as delivered in Abbé de la Caille's recital; the third contains the computed values of the parallax at different times, taking that of the maximum 9"; the fourth column gives observations reduced to the mean, by applying the parallax computed in the third column to the observations in the second; which quantities ought all to agree together, if the observations were liable to no error, and the parallax was rightly assumed: but, taking a mean of them all, the last column shews how much each of them differs from that mean,

mean, which, in general, is very small, and scarce exceeds $2''$, except in two observations, in one of which it amounts to $3''$, and the other to $4''$: but these differ as much from the mean of the six other observations made at the same season of the year. Thus, assuming a parallax, the observations will be found to agree as well with that supposition, as they do with one another. But if the observations are considered in themselves, without any allowance for parallax, they will differ sensibly from one another: nor is this difference to be found only in two observations, in which case, it may easily be attributed to the account of the unavoidable errors; but five observations in July opposed to two in December and January, make the zenith distances vary $8''$ in the direction, which a parallax ought to produce.

I am aware, that it may be objected, that two observations made in the winter season in December and January, at one of the maxima of the parallax, are too few to determine a point of such consequence, and readily agree, that the argument is weakened in proportion to the paucity of the observations: but then, it should also be considered, that the observations made in March and April concur with the rest in supporting the supposition of a parallax; and, upon the whole, the observations will, perhaps, be judged to afford a sufficient presumption of the existence of a parallax, to encourage the undertaking a careful trial. It is upon the strength of this presumption, principally, that I have ventured to lay these thoughts before the Royal Society, whose members may justly claim the honour of having made the greatest attempts towards demonstrating this important, but yet undetermined point in astronomy.

LXXIX. *Farther Experiments in Electricity;*
by Mr. Benjamin Wilson, F. R. S.

*To the Right Honourable the Earl of Macclesfield,
President of the Royal Society.*

My Lord,

Read Nov. 13, 1760. **I**N my letter upon the *Tourmalin*, which I had the honour of communicating to the Royal Society in December last, there are some experiments to shew, that glass is permeable by electricity. I have since made others of the like kind, which seem to favour the same opinion.

The high regard your Lordship has always shewn for experimental philosophy, encourages me to lay before you these experiments, with a few others, of a very curious nature.

But I beg leave, first, to take notice, that our electrical apparatus is much improved, by the discovery of Father *Windelinus Ammersin*, of *Switzerland*, who, in a Latin treatise published in 1754, has shewn us, that wood, properly dried, till it becomes very brown, is a non-conductor of electricity*; and recommends boiling the wood in linseed oil, or covering it all over with varnish, after being dried, to prevent any return of moisture into its pores. He

* It appears, from the Philosophical Transactions, so early as the year 1747, that Dr. *Watson* having occasion to support a long wire, in the experiment made near Shooter's Hill, with a view to determine the velocity of the electric fluid, used stakes of dry wood, (which, he tells me, were baked) to prevent the electric fluid from escaping into the ground.

adds,

adds, that wood so treated seems to afford stronger appearances than even glass *.

The following experiments were made, with the assistance of wood thus prepared.

Being provided with a large square of glass, polished on both sides, and fixed upright upon one edge, I placed, for a conductor, a slender piece of ivory, about one foot long, having one end within two tenths of an inch from the center of the glass: at the other end were suspended two small balls of pith, by threads four inches long. The ivory was supported horizontally by a stand made of the *prepared wood*. When the glass was made a little warmer than the external air, my finger rubbed that side thereof which was farthest from, and opposite to, the ivory. Upon doing this, the two sides of the glass were electrified *plus*, as were likewise the balls; which continued *plus*, even after they were removed from the glass into any part of the room.

From the appearance of a *plus* electricity in the ivory, as well as on the two sides of the glass, the fluid must have flowed from the finger into the glass, and part of it into the ivory: otherwise, permit me to ask how the ivory should be electrified *plus*?

That the fluid really flowed from the finger into the glass, may be inferred from the following experiment.

A piece of silver, being fixed upon a slender rod of prepared wood, I rubbed the same glass with it, as I had done before with the finger; upon which the

* See the Abbé Nollet's Letters upon Electricity, published at Paris in 1760.

silver was electrified *minus*, and both sides of the glass, with the conductor and balls, *plus*.

There are, therefore, certain circumstances under which the electric fluid passes through glass; I repeat in *certain circumstances*, my Lord, because in others, for instance, the *Leyden bottle*, the fluid does not pass through the glass, but electrifies one side *plus*, and the other side *minus*, as my friend Dr. *Franklin* hath shewn, in his letters upon electricity.

This difference, which is caused by the vitreous electricity in both cases, may perhaps be explained from the *surrounding medium* *, and the different degrees of the *power* with which the electric fluid endeavours to enter a body.

For the *resistance* owing to the *surrounding medium* in a polished plane, without covering, is not overcome, but by bringing the *power* near the glass, whereby the glass is electrified *plus* on both sides; whereas, when the glass is *properly covered*, and the *power* brought upon it, the covering conducts the fluid equally to all the parts on that side, where it accumulates, that is, electrifies it *plus*; whilst the opposite covering carries off to the communicating earth an equal quantity of the fluid naturally belonging to the glass, that is, electrifies it *minus*.

The same glass, without any covering, will be rendered *minus* on both sides, by removing the *power* to a greater distance, by which it is diminished, and the quantity of intermediate air, and consequently of *resistance*, increased.

* See the former paper upon the *Tourmalin*, p. 329. of this Volume, Part I.

The *power* therefore which causes the *minus* effect, is less than the *power* which causes the *plus* effect: and that this *minus* effect arises intirely from the *repulsive power* of the fluid, appears from this, that part of the fluid naturally belonging to the glass, is driven off, and electrifies *plus* other bodies placed beyond it.

From hence I collect, that the *three different* effects, *viz.* the electrifying glass *plus on both sides*; or *plus on one side*, and *minus on the other*; or, lastly, *minus on both sides*; are occasioned by the *different degrees* of the *same power* and *resistance* in the respective experiments with the *same glass*.

If, on the other hand, the three last experiments are made with the *minus*, instead of the *plus* electricity, the appearances, and consequently the reasoning upon them, are inverted: for the *plus* appearance, in that case, must be owing to the tendency of the fluid from the earth, air, and other neighbouring bodies, towards the opposite side of the glass, where the *power* acts, which causes the *minus* appearance.

This regular conversion serves to confirm the truth of the principles assigned.

And here, with your Lordship's leave, I shall set down some farther experiments, most of which have been made with a view to shew, that a *plus* electricity may be produced by means of a *minus* electricity.

Having electrified the inside of a large *Leyden* bottle *plus*, by means of a conducting-wire from an excited glass globe, I set it upon a stand of prepared wood, and took away the conducting-wire; after which, the mouth of the bottle was closed with a

stopple of glass. Then the pointed end of the ivory conductor was brought opposite to the middle of the bottle, and about two inches from it. Upon doing this, the balls were electrified *minus*; and more so, as the ivory was moved nearer the bottle in an horizontal direction.

But, on removing the ivory to a greater distance, the *minus* electricity decreased; and, at a certain distance, there was not any sign thereof remaining: and when the distance was increased to about eighteen inches from the bottle, a *plus* electricity appeared, which continued, even after the ivory was removed intirely away.

After unelectrifying the bottle, it was set again upon the stand of wood; and upon another of the same kind, was laid a large iron poker, so that the pointed end was about two inches from the outside coating, and much nearer the bottom than the top of the bottle. I chose the lower part thereof, for no other reason, than to have the poker out of the reach of any effect that might happen from the conducting-wire, which communicated with the machine, and the inside coating of the bottle. The other end of the poker, being a large round knob, was about one inch from a rod of iron, which communicated with the earth. Under these circumstances, when the inside of the bottle was electrified *plus*, the outer coating became *minus*, as usual.

It is to be observed here, that the *plus* appearance in the poker was caused by that portion of the fluid, which was driven off from the outside of the bottle, by the repulsive force of the fluid conveyed into the bottle. And this confirms the reasoning upon the remarkable

markable experiments related in the treatise published by Dr. Hoadly and myself. See p. 27 to 34, and 46, 47.

Instead of electrifying the inside of the bottle, in the last experiment, *plus*, I electrified it *minus*, with the prepared wood, without any other change of circumstances. In this case, the outside of the bottle was *plus*, as usual, and the poker *minus*. The fluid, therefore, which caused the *plus* electricity on the outside of the bottle, must, in part at least, have flowed from the poker; because there was no other open communication with the outside of the bottle and the earth to supply it.

With a cylinder of baked wood, which shewed stronger signs of electricity, than I have yet been able to produce from glass, the balls hanging to the ivory were electrified *minus*, at the distance of four feet, or more, by holding the cylinder over the middle of the ivory, and continuing it there: and on moving it nearer, they were more strongly electrified *minus*; but the same cylinder, on removing it back again to the distance of two or three feet, or more, electrified the balls *plus*.

When another conductor, for instance metal, without edges or points, was used instead of the ivory, and without any thing hanging from it, the same cylinder held over the metal, as was done in the last experiment over the ivory, at the distance of two feet, produced a *plus* electricity: and this was rendered weaker, as the cylinder was moved nearer; but by lessening the distance to about one foot, the *minus* electricity took place.

In these cases, the *plus* appearance arose from the earth, air, or other neighbouring bodies.

When the preceding experiments were first made, I was a little embarrassed with the uncertain appearances of a *plus* electricity at one time, and a *minus* at another, in the same experiment: but, by repeated trials and observations, I have found that a *plus* or *minus* electricity may be produced at pleasure, by carefully attending to the following circumstances, *viz.* the form of the bodies, their sudden or gradual removal, and the degrees of electrifying.

I shall now proceed to acquaint your Lordship with some other circumstances of as nice a nature, where the slightest, and almost imperceptible differences in the *position*, or in the *force* of the *friction*, of two bodies, produce in either of them, the *plus* electricity at one time, and the *minus* at another.

Such are the effects of this subtle and active fluid, when the experiments are carefully made; and therefore, they require the most scrupulous attention, to trace out the causes which occasion them.

Sealing wax and silver were the bodies used in the first two experiments; but many other substances seem to perform as well. The sealing wax was clean, and undisturbed by any friction whatever, but that of the air surrounding it; and had been so for some hours. The silver was fixed to a piece of the prepared wood, which was also preserved from friction for the same length of time. Then taking one of these substances in each hand, the silver being at that end of the wood the farthest from the hand, I laid the smoothest part of the silver upon the sealing wax, and moved it along the surface gently, *once only*, and with a very *slight pressure*; after which, the silver was electrified *plus*, and the wax *minus*.

On repeating the experiment, with equal care, and in the same manner, except that the *smooth side* of the silver was a little inclined, so that the *edge* thereof pressed against the wax; the silver, after moving it as before, was electrified *minus*, and the wax *plus*; which is contrary to what was observed in the last experiment.

These opposite effects, occasioned by the different applications of the flat, or edge of the silver, seem to arise from an alteration made in the surface of the wax, by *destroying* the *polish* in one case, and not in the other: and, in this respect, resembles the polished and rough glass mentioned in the letter upon the Tourmalin. See p. 328. of this Volume, Part I. Experiment 24.

Upon making use of prepared wood, instead of wax, and employing different degrees of pressure in the friction, with the *same edge* of the silver, I produced the like appearances; the least pressure causing a *plus*, and the greatest pressure a *minus* appearance in the silver.

A flat piece of steel, well polished, and the edges rounded off, afforded the same appearances, by only applying the *flat surface* to the wood; but it required more *pressing* to produce the *minus* effect in this case, than it did in the former, where the *edge* was concerned.

Whether the reason offered above for explaining these last curious appearances be true or not, I do not venture to affirm, for want of farther experiments; but thus much may be safely advanced, that we have learned to produce at pleasure the *plus* or *minus* electricity from the same bodies, by attending to the manner of their *application* and *friction*.

Mr. Dufay, and the Abbé Nollet, have indeed observed an inconstancy in the effects, when they rubbed sealing wax; but for want of the cautions above-described, in the choice and treatment of the bodies, they were not able to answer for the event of any experiment. The Abbé, in particular, who has taken remarkable pains to find out from whence this uncertainty arises, acquaints us, in the *thirteenth letter*, with his difficulties, and how much it perplexed him to see, within an hour, in the same place, and with the same instruments, the effects changed, and become altogether contrary to those he had before observed. In another part of the same letter, he says:

“ Je me suis flatté plus d’une fois d’avoir saisi les
 “ circonstances qui faisoient tourner à coup sûr le ré-
 “ sultat de l’expérience d’un côté ou de l’autre; mais
 “ la suite m’a fait connoître que je m’étois trompé,
 “ ou qu’il manquoit encore quelque chose à ma dé-
 “ coverte.”

I might add other quotations of the same kind, but, as it would take up too much of your Lordship’s time, I shall beg leave to refer to the letter itself.

From the experience now obtained, it may not be improper to observe farther, that sealing wax of different hardness, will occasion a difference in the effects, the hardest being always *plus*, and the softest *minus*; which agrees very well with the rule laid down in the letter upon the Tourmalin, p. 331,
 “ that where electric appearances are produced by
 “ the rubbing of any two polished bodies together,
 “ that body, whose substance is hardest, and electric
 “ power strongest, will be always *plus*, and the softest
 “ and weakest always *minus*.” Now we find that
 the

the Dutch wax is softer than the English, and the Irish harder: and it is not improbable that the French wax, which I suppose the *Abbé Nollet* used, may likewise be different.

Whoever, therefore, may be desirous of trying the preceding experiments, with a view to make them succeed, as they are here set down, must use the *best English sealing wax*.

I shall conclude with an experiment made by my friend *Mr. Hamilton*, professor of philosophy in the university of Dublin, as it seems to illustrate the doctrine of *resistances*, at least, so far as respects the air.

Let a slender brass, or iron wire, five or six inches long, and finely pointed at each end, be fitted in the middle, with a brass cap, void of angles; then let half an inch at each extremity be bent in opposite directions, till they are perpendicular to the rest of the wire, and in such a manner, that when the wire is suspended, by means of its cap, on a point of metal, it may lie in a plane parallel to the horizon. The pointed metal, which supports this wire, must be two or three inches long, and have its other end fixed into a small block of wood. Now, if this block, with a wire suspended, be set upon an electrified body, the wire will turn round with a very great velocity, moving always in a direction contrary to that in which the electric fluid issues from its points, without having any conducting substance near it, save that of the air: and if the wire be made to turn round by any other force, in the opposite direction, so that its points go foremost, it will, when electrified, soon
be

be deprived of that motion, and made to turn round the contrary way.

This experiment, he says, was contrived, in order to try whether the electric fluid, which issues so freely from pointed bodies, would have any effect to move these bodies by its reaction; and that it has such an effect, seems sufficiently manifest from the event.

Mr. Hamilton apprehends, that the electric particles, by their elastic force, issue directly forwards from the points, and endeavour to expand themselves; but meeting with some resistance from the air, force the wire to move backward in a contrary direction, much in the same manner that a Catherine-wheel is made to turn round in a direction, contrary to that in which the small rockets affixed to its periphery discharge themselves. And therefore, he is inclined to think, that it might be made use of as an electrometer, by having it to turn round in a plane perpendicular to the horizon, and loading the wire with small weights near one of its extremities, which will be raised to a greater distance from the perpendicular line, as the electric fluid is stronger.

I am, my Lord,

Your Lordship's

most obliged

and obedient servant,

Benj. Wilson.

Great Queen-Street,
Nov. 5, 1760.





J. Moxley sc.

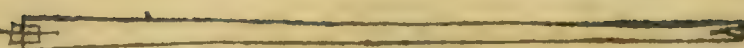
LXXX. *A Letter to Mr. Benjamin Wilfon,
F. R. S. concerning Electricity; from Mr.
Thorbern Bergman, of Upsal.*

Amplissime atque Celeberrime Domine,

Read Nov. 20,
1760.

NON mireris, Vir Celeberrime, me
physicis addictum Scientiis tuam
quærere amicitiam, cujus nomen jam diu in hoc Sci-
entiarum genere orbi erudito inclaruit. Præterea Do-
minus Professor Ferner me certiores fecit Te non
ægre laturum mecum *commercium literarium* insti-
tuere de *rebus electricis, aliisque curiosis physicis*. Plu-
rima enim apud Vos quotidie inveniuntur, quorum
tempestiva cognitione lætatur quivis Philosophus na-
turalis; forte etiam varia apud nos prodeunt, quæ
Anglis haud ingrata spero. Itaque constitui secun-
dum tuum nutum, eos Philosophiæ naturalis pro-
gressus, quos in *Suecia* capit, notos facere, à Te vi-
cissim expectans eorum notitiam, quibus in *Anglia*
locupletatur. Lingua utor Romana, quippe quæ mihi
notior Anglica, cujus genius ab exterraneo, qui in
Anglia nunquam fuit, vix ita addisci potest, quin
multa vitia irrumpant, quæ Anglo inter legendum
nauseam movent.

Dominus *Franklin* in §. 28. *tertiæ epistolæ mira-*
tur glaciei frustum non transmittere commotionem
electricam, cum aqua idem perfecte præstet: sed feci
nonnulla experimenta, quæ monstrant parvam aquæ
quantitatem nec huic fini sufficere. Scilicet capiatu-
rus vitreus trium vel quatuor pedum, qualis barometris
inservit: hic aqua impleatur, et claudatur utrumque
extremum subere perforato filo metallico aquam pau-
lulum intrante, uti figura adposita indicat.

A  A

Si jam duo homines in circulo explosorio constituti teneant extrema filorum metallicorum A, A, et tentetur evacuatio, non tamen perfecta evenit, nam vix carpus et rarissime flexura cubitus hac methodo concutitur, licet deinde idem vas absque ulteriori cumulatione tantum contineat electricitatis, ut more consueto evacuatum pectus valide ferire possit. Aucta tubi capacitate, magis transit, usque dum ita increverit, ut plenarium transitum permittat, nec impedit aquæ quantitas justo major, quod testantur commotiones per lacus et flumina propagatæ. *Conveniunt itaque aqua et glacies in eo, quod parvæ quantitates concussionem ægre tramittant.*

Hinc suspicatus sum *magnam glaciei quantitatem faciliorem concedere transitum, quod etiam experientia comprobavit, etsi adhuc non majus adhibui frustum quam quo commotio flexuram cubitus attigit. Præterea ex æqualibus aquæ et glaciei quantitatibus, glacies minus transmittit.*

Num *Auroras Boreales* uti *phænomena electrica* respicias, nec ne, nescio; *mibi saltem adhuc incertum videtur.* Interea *singularem observationem* æstate præterlapsa factam commemorabo. *Die xx Julii hora æ post meridiem vidi in N. W. nubem AB horizonti insistentem, in qua copiosæ et crebræ fulgurationes ab A versus B progrediebantur, absque tamen ullo tonitru, ne quidem minimo murmure. C et D erant nubeculæ rariores forte cum magna non cohærentes, nam in eis nulla vidi fulgura. [Vide Tab. XXI.] Hora xi circiter adparuit Aurora Borealis suo culmine ultra zenith progrediens, et pluries arcus extremitatibus nubem attingens. Etsi probe examinarem, nullam*

nullam aliam communicationem inter nubem et auro-ram observare potui, nam 1°. in D, cui institit alterum crus, nullam vidi fulgurationem: 2°. dum in nube fulguraret auroræ splendor, nullam variationem pati videbatur. Hora xii desierant fulgura, et nubes ita erat dilatata, ut totum fere hemisphærium visibile occuparet.

Præterito, et *hoc quoque anno, multas elegantissimas observari Auroras Boreales.* Si quispiam *tales Londini* attentis consideraverit oculis, per tuum favorem *notitiam* me obtenturum spero, et *quando et quomodo* ibi apparuerint, ut sciam num fuerint eædem, quas hic vidi. Saltem observationum collatione certius hujus phænomeni altitudo perpendicularis determinaretur. Si tibi placuerit, alia occasione lubenter meas transmittam observationes.

Interea favori tuo me commendo permanfurus

Amplissimi atque Celeberrimi Nominis Tui

Cultor observantissimus,

Dabam Upsalæ,
17 Oct. 1760.

Thorbern Bergman.

Quomodo electricitas cumulari possit in aliis quam viteris materiis, ex. gr. lacca sigillatoria, sulphure, papyro, ære, &c. adeo ut commotio electrica ex eis obtineatur, non haud ita pridem in actis Academiæ Scient. Stokholmensis docuit Carolus Vilcke Physices Lector; Methodus forte tibi non ignota.

LXXXI. *Some Considerations on a late Treatise intituled, A new Set of Logarithmic Solar Tables, &c. intended for a more commodious Method of finding the Latitude at Sea, by Two Observations of the Sun; by H. Pemberton, M. D. R. S. Lond. et R. A. Berol. S.*

Read Nov. 20,
1760.

AS it happens not unfrequently at sea for the unseasonable intervention of clouds to prevent the ordinary method of determining the ship's latitude by the sun's meridian altitude, even when it is of primary consequence, that the true latitude should be known; a problem for remedying this disappointment is stated in many treatises of navigation, for finding the latitude of a place by any two altitudes of the sun, with the interval of time between them.

A problem similar to this is proposed, and solved instrumentally upon a globe, by a very early writer, Petrus Nonius (*a*), namely, to find the latitude by two altitudes of the sun, and the angle made by the azimuth circles passing through the sun, when the altitudes are taken. And since more commodious and accurate instruments for measuring time have been invented, than were known to this author, the other problem has been proposed for the same pur-

(*a*) In Libr. De Observ. Regul. et Instrum. Geometr. lib. ii. c. 14.

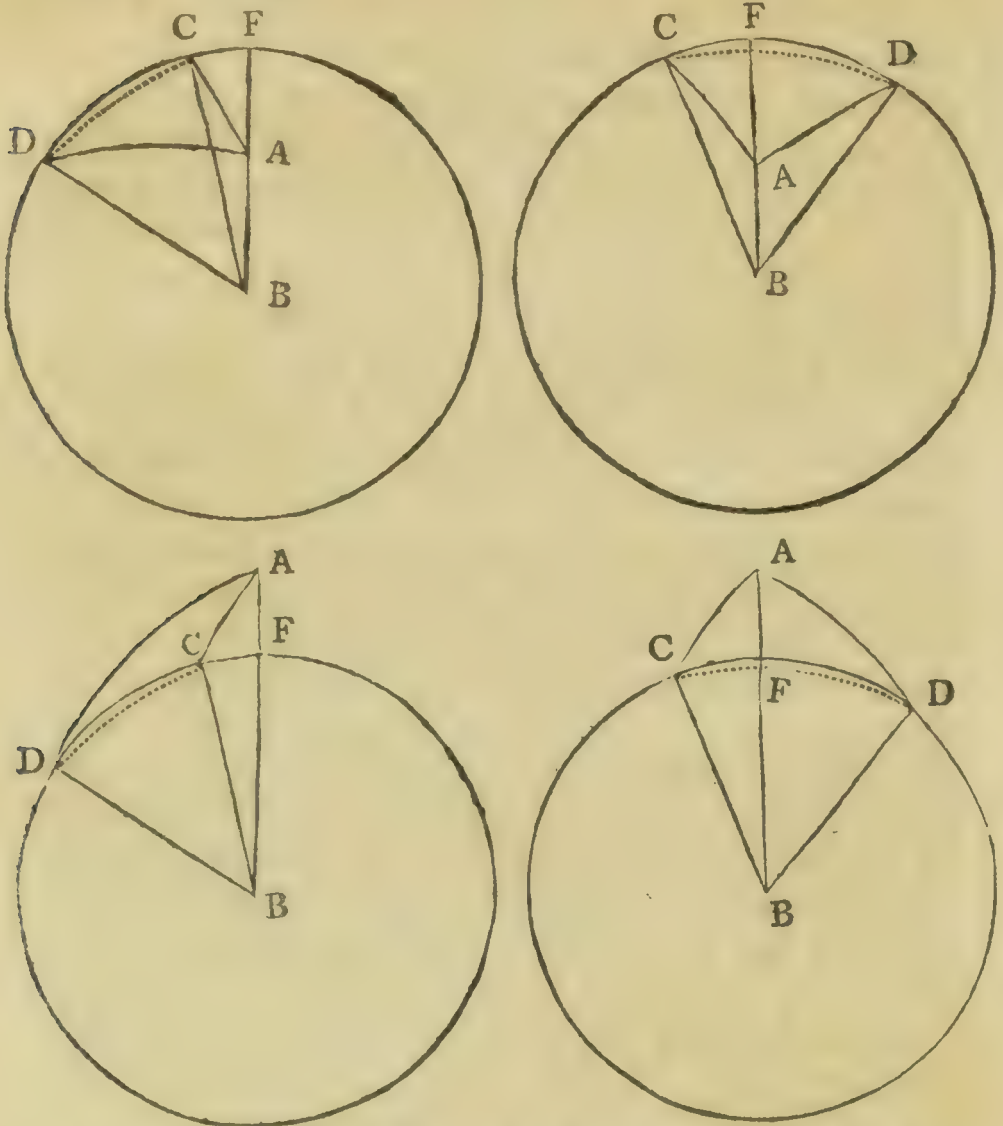
pose, of which a construction upon the principles of the stereographic projection of the sphere is exhibited by Mr. Collins, in his Mariner's Plain Scale new planed (*b*). And as the direct method of solving both these problems by numbers requires a diversity of trigonometrical operations, a set of tables has lately been published for a more compendious way of computation in the problem, where the interval of time is given, whereby the ship's true latitude may be very expeditiously derived from the ship's dead reckoning, provided the observations are made within certain limits of time.

But however worthy of notice this method may be, new tables for the purpose are altogether unnecessary. It consists of two parts: the first computes, from the latitude exhibited by the dead reckoning of the ship, the distance from noon of the middle time between the observations, and thence the time of either: the second operation computes, from one of these observations, what should be the sun's meridian altitude, had the ship's reckoning given the true latitude; but if the latitude assumed from that reckoning is erroneous, the altitude thus computed will not be conformable to it; however, if the times for the observations are properly chosen, it will much better agree to the true latitude, and thereby the assumed latitude may be more or less corrected.

But both these operations are an immediate consequence from the proposition in spherical trigonometry, usually delivered under the name of the fourth axiom, which is this; That the square of the radius is to the rectangle under the sines of the sides con-

(*b*) Book iii. p. 35.

taining any angle, as the versed sine of that angle to the difference between the versed sines of the third side, and of the difference between the sides containing the angle.



Hence if A be the zenith of any place; B the elevated pole; C, D two places of the sun in the parallel of declination DF; by this proposition in the triangle ABC,

Rad.^a : fin. AB × fin. BC (cof. lat. × cof. decl. ☉)
 :: verf. f. ABC : verf. f. AC — verf. f. AF ;

And, in like manner, in the triangle ABD,
 Rad.^a : cof. lat. × cof. decl. ☉ :: verf. f. ABD
 : verf. f. AD — verf. f. AF ;

Consequently,
 Rad.^a : cof. lat. × cof. decl. ☉ :: verf. f. ABD — verf. f. ABC
 : verf. f. AD — verf. f. AC.

But $\frac{1}{2}$ rad. × $\overline{\text{verf. f. ABD} - \text{verf. f. ABC}}$ is =
 fin. $\frac{ABD + ABC}{2}$ × fin. $\frac{ABD - ABC}{2}$;

Therefore,

Rad.^a : cof. lat. × cof. decl. ☉ :: fin. $\frac{ABD + ABC}{2}$
 × fin. $\frac{ABD - ABC}{2}$: $\frac{1}{2}$ rad. × $\overline{\text{verf. f. AD} - \text{verf. f. AC}}$.

And this is the first operation in the treatise, these remarks concern. For in the rectangle, the fin. $\frac{ABD + ABC}{2}$
 × the fin. $\frac{ABD - ABC}{2}$, the first side, when the observations are both made on the same side of noon, and the second side of this rectangle, when one observation is before noon, and the other after, is the sine of the distance of the middle time between the observations from noon, and the other side is the sine of half the distance between the observations. And in the new tables one column exhibits the arithmetical complement of the logarithmic sine of half the elapsed time, and the next, the logarithm of twice the sine of the middle time.

But moreover, $\frac{1}{2}$ rad. × $\overline{\text{verf. l. AD} - \text{verf. l. AC}}$
 being equal to fin. $\frac{AD + AC}{2}$ × fin. $\frac{AD - AC}{2}$, the operation may be expressed more commodiously thus,
 Rad.

$$\text{Rad.}^{\text{a}} : \text{cof. lat.} \times \text{cof. decl. } \odot :: \text{fin. } \frac{\text{ABD} + \text{ABC}}{2} \\ \times \text{fin. } \frac{\text{ABD} - \text{ABC}}{2} : \text{fin. } \frac{\text{AD} + \text{AC}}{2} \times \text{fin. } \frac{\text{AD} - \text{AC}}{2} ;$$

now requiring the common logarithmic fines only.

Again, the square of the radius being, as above, to cof. lat. $\times$ cof. decl. $\odot$, as the vers. f. ABC to vers. f. AC — vers. f. AF, or as vers. f. ABD to vers. f. AD — vers. f. AF, these analogies express the second operation, which, in this treatise, is unnecessarily confined to the lesser angle ABC. The column in the table intituled Rising consists of logarithmic versed fines, which may be applied to either of the angles ABC or ABD promiscuously; for here AF being equal to the excess of BC or BD above the arch BA, assumed for the complement of the latitude, the arches AC and AD, in the preceding analogies, will be the complements of the altitudes observed, if the latitude were truly assumed, otherwise not; but the difference of their versed fines, will however be equal to the difference of the versed fines of the complements of the true latitudes; for this is supposed in the first operation. Therefore, if one of the fourth terms of these analogies be deducted from the versed fine of the complement of the greater altitude, or the other from the versed fine of the complement of the lesser, the remainder will be the same, and exhibit a versed fine for the complement of the sun's altitude different from AF, when BA is assumed different from the true latitude, and nearer to the truth, if the times for the observations are properly chosen.

But farther, the two preceding analogies may be reduced to these;

Rad.

$$\text{Rad.}^q : \text{cof. lat.} \times \text{cof. decl. } \odot :: \overline{\text{fin. } \frac{1}{2} \text{ ABC}}^q \\ : \frac{1}{2} \text{ rad.} \times \overline{\text{verf. f. AC} - \text{verf. f. AF}},$$

And

$$\text{Rad.}^q : \text{cof. lat.} \times \text{cof. decl. } \odot :: \overline{\text{fin. } \frac{1}{2} \text{ ABD}}^q \\ : \frac{1}{2} \text{ rad.} \times \overline{\text{verf. f. AC} - \text{verf. f. AF}};$$

Also,

$$\text{Verf. f. AC} - \text{verf. f. AF} = \text{cof. AF} - \text{cof. AC}, \text{ and} \\ \text{verf. f. AD} - \text{verf. f. AF} = \text{cof. AF} - \text{cof. AD}.$$

And thus the common tables of fines will supply the use of this column of the new tables, as well as the preceding.

The first example of this treatise, wrought by the common tables.

Here the declination of the sun is stated at $11^{\circ} 17'$ N. the altitude first taken, being the lesser, $46^{\circ} 55'$, the greater altitude $54^{\circ} 7'$, and the difference of time between the observations $1^h 25^m$, or 85^m ; which, divided by 4, gives $21^{\circ} 15'$ for the arch of the equinoctial corresponding.

Here the observations are both before noon; therefore, in fig. 1. the latitude being assumed $46^{\circ} 50'$,

For the first operation.

AC =	35	53	0	
AD =	43	5	0	
Their sum =	78	58	0	
Their difference =	7	12	0	
Sine of $\frac{1}{2}$ sum (39 29 0)				9.80336
Sine of $\frac{1}{2}$ difference (3 36 0)				8.79789
Arith. com. of the sine of $\frac{1}{2}$ CBD (= 10 37 30)				0.73429
Arith. com. of the sine of BF, or the cof. of the decl. } of the sun, - - - - -				0.00848
			Sum	19.34402
			Cof. of the lat. assumed	9.83513
			Diff. = fin. $\frac{\text{ABD} + \text{ABC}}{2}$	9.50889

For the second operation.

$$\frac{ABD + ABC}{2} = 18^{\circ} 49' 50''$$

$$\frac{ABD - ABC}{2} = 10^{\circ} 37' 30''$$

$$ABC = 8^{\circ} 12' 20''$$

$$\text{Sine of } \frac{1}{2} ABC (4^{\circ} 6' 10'') \left\{ \begin{array}{l} 8.85458 \\ 8.85458 \end{array} \right.$$

$$\text{Cof. of decl. } \odot \quad 9.99152$$

$$\text{Cof. of the lat. assumed} \quad 9.83513$$

$$\text{Sum} - 30.00000 = \log. \text{ of the } \frac{\text{verf. f. AC} - \text{verf. f. AF}}{2} \quad 7.53581$$

$$\text{Verf. f. AC} - \text{verf. f. AF} \quad 0.006868$$

$$\text{Natural cof. of AC, the complement of the greater altitude, } - - - - - \left\{ \begin{array}{l} 0.810212 \\ 0.817080 \end{array} \right.$$

$$\text{Natural cof. of } 35^{\circ} 12' 23'' \quad 0.817080$$

$$\text{Or, } \frac{ABD + ABC}{2} + \frac{ABD - ABC}{2} \text{ being } = ABD,$$

$$\text{Sine of } \frac{1}{2} ABD (14^{\circ} 43' 40'') \left\{ \begin{array}{l} 9.40522 \\ 9.40522 \end{array} \right.$$

$$\text{Cof. of the decl. of the sun} \quad 9.99152$$

$$\text{Cof. of the latitude assumed} \quad 9.83513$$

$$\text{Sum} - 30.00000 = \log. \text{ of the } \frac{\text{verf. f. AD} - \text{verf. f. AF}}{2} \quad 8.63709$$

$$\text{Verf. f. AD} - \text{verf. f. AF} \quad 0.006720$$

$$\text{Natural cof. of AD, the complement of the lesser altitude, } - - - - - \left\{ \begin{array}{l} 0.730361 \\ 0.817081 \end{array} \right.$$

$$\text{Natural cof. of } 35^{\circ} 12' 23'', \text{ as before} \quad 0.817081$$

Thus the meridian distance of the sun from the zenith is computed at $35^{\circ} 12' 23''$, and this added to the sun's declination $11^{\circ} 17' N$. gives $46^{\circ} 29' 23''$ for the latitude, different from that assumed, and nearer to the true latitude; which, by a second operation

operation with this latitude now found, will approach still nearer to the truth, thus :

The sum of the four first logarithms in the first of } 19.34402
the two former operations, - - - - - }

Cofine of $46^{\circ} 29' 23''$	9.83789
Sine of $\frac{ABD + ABC}{2} = 18^{\circ} 42' 25''$	9.50613
$\frac{ABD - ABC}{2} = 10^{\circ} 37' 30''$	
$ABC = 8^{\circ} 4' 55''$	
$\frac{1}{2} ABC = 4^{\circ} 2' 27\frac{1}{2}''$	{ 8.84800
	8.84800
	9.99152
	9.83789
Log. $\frac{\text{verf. f. } AC - \text{verf. f. } AF}{2}$	7.52541
Verf. f. $AC - \text{verf. f. } AF$	0.006704
Natural cof. of AC , as before,	0.810212
Natural cof. of $35^{\circ} 13' 21''$	0.816916

The declination of the sun added to this, amounts to $46^{\circ} 30' 31''$, the true latitude, as directly computed, being $46^{\circ} 30' 19''$.

However, it must not be expected, that this method of computation will always converge thus expeditiously to the true latitude. Had these observations been made about 3 hours sooner in the day, and with the same interval between them, if the first altitude of the sun had been found $18^{\circ} 55'$, and the second $33^{\circ} 11'$, the latitude computed would come out wider from the truth, than that assumed, one exceeding, and the other falling short of the true latitude, which will lie between them, and is nearly the same, as above. But if the lesser altitude had

been $29^{\circ} 13'$, and the greater 40° ; in 9 degrees of north latitude the interval between the observations would have been nearly the same, as before. But if this latitude be sought by any latitude assumed near it, the latitude computed according to the method above, will fall more remote from the truth, than that assumed, and err the same way.

In general, the error in the latitude assumed will bear to the error in the latitude computed, nearly the ratio compounded of that of the rectangle under the radius and the cosine of the distance from noon of the middle point of time between the two observations to twice $\sin. \frac{1}{2} ABC \times \sin. \frac{1}{2} ABD$, and the ratio of $\text{rad.} \times \sin. AF$ to $\sin. BC \times \cos. AB$; insomuch that the distance of the middle point of time between the two observations from noon is to be considered, as the limit, where these computations shall cease to converge, when the rectangle under the radius, and the cosine of this distance from noon shall be to twice $\sin. \frac{1}{2} ABC \times \sin. \frac{1}{2} ABD$, (or the cosine of this distance to the difference between the versed sine of this distance from the versed sine of half CBD) as $\sin. BF \times \cos. AB$ to $\text{rad.} \times \sin. AF$. And the errors in the assumed and computed latitudes fall on different sides of the true latitude, when both the observations are on the same side of noon, and the zenith lies between the meridian sun and the elevated pole; or when the observations are one before, and the other after noon, if the meridian sun passes between the zenith and the elevated pole: otherwise, they fall on the same side of the true latitude.

However,

However, the direct method of computation, by the assistance of the natural fines, will not be so much more operose than this compendium, as may at first sight be imagined. For the arch of a great circle being drawn through C and D, forming the triangle BCD, if the logarithmic cosine of BC or BD be added to the logarithmic tangent of half the angle CBD, and the logarithmic sine of BC or BD be added to the logarithmic sine of half this angle, the first sum is the logarithmic cotangent of the angle BCD, and the second the logarithmic sine of half CD, the base of the triangle. Then, in the triangle ACD, from the sides, now all given, is to be computed the angle ACD, the difference between which, and the angle above found BCD, is the angle BCA, when the zenith lies between the pole, and the great circle through C, D; but when the zenith lies beyond that circle, the angle BCA is either the sum of those angles, or the supplement of that sum to a circle. And, in the last place, if twice the logarithmic sine of half this angle, and the logarithmic sines of BC and AC are added together, the sum, after thrice the logarithm of the radius has been deducted, is the logarithm of half the excess of the natural cosine of BC ∞ AC above the natural cosine of AB, or the natural sine of the latitude, according to the trigonometrical axiom, which has been above referred to; for $\text{rad.} \times \frac{1}{2} \text{ vers. f. BCA}$ is equal to $\overline{\text{fin. } \frac{1}{2} \text{ BCA}}^q (c)$.

(c) Vide Neper. Mirif. Canon. Construct. Edinburg. 1619, p. 59. The

The operation for the preceding example will be this :

Tang. $\frac{1}{2}$ CBD — 9.273225 Cof. BC — 9.291504 Cotang. BCD — 9.564729 BCD = $87^{\circ} 53' 52''$	Sine $\frac{1}{2}$ CBD — 9.265714 Sine BC — 9.991524 Sine $\frac{1}{2}$ CD — 9.257238 $\frac{1}{2}$ CD = $10^{\circ} 25' 2\frac{1}{2}''$
--	--

AD = $\begin{matrix} 0 & ' & '' \\ 43 & 5 & 0 \end{matrix}$ AC = $\begin{matrix} 35 & 53 & 0 \end{matrix}$ CD = $\begin{matrix} 20 & 50 & 5 \end{matrix}$ Sum = $\begin{matrix} 99 & 48 & 5 \end{matrix}$ $\frac{1}{2}$ Sum = $\begin{matrix} 49 & 54 & 2\frac{1}{2} \end{matrix}$ $\frac{1}{2}$ Sum — AD = $\begin{matrix} 6 & 49 & 2\frac{1}{2} \end{matrix}$	— 0.232001 } ar.co. of the fines. — 0.448950 } — 9.883621 } fines. — 9.074466 } Sum — 19.639038 $\frac{1}{2}$ Sum — 9.819519
---	---

This half sum is the logarithmic cofine of half ACD; therefore, half ACD is = $48^{\circ} 42' 11''$, and its difference from $\frac{1}{2}$ BCD will give $\frac{1}{2}$ ACB = $4^{\circ} 45' 15''$.

Sine of $\frac{1}{2}$ ACB	{ 8.918444
	{ 8.918444
Sine of AC —	9.767999
Sine of BC —	9.991524
0.0039483 — log. —	7.596411
2	

0.0078966

0.7333345 — natural cofine of BC — AC, that is, of $42^{\circ} 50'$.

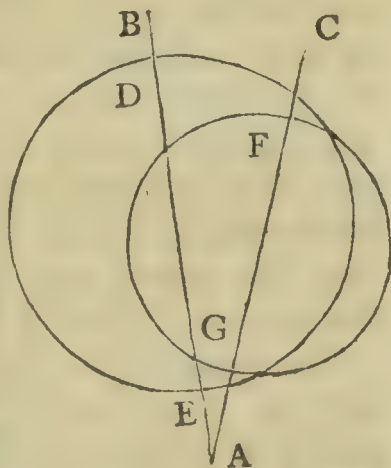
0.7254379 — natural cofine of AB, or fine of the latitude.

Therefore, the latitude is $46^{\circ} 30' 19''$.

But farther, as in the cases, where the above approximation takes place, it is an advantage, that the assumed latitude should be taken as near the true one,

one, as can be, the dead reckoning of the ship need not be intirely relied on for that purpose; for the instrumental construction proposed by Mr. Collins (*d*) will very readily give the true latitude, as nearly as the instruments used can express: and his method may be described thus.

Two straight lines AB, AC being drawn in an angle corresponding with the distance in time between the observations, and in one of the lines, as AB, the lengths AD, AE being taken from any scale of tangents, one equal to the tangent of half the sum, and the other equal to the tangent



of half the difference of the distance of the sun from the pole, and from the zenith of the first observation, and also the points F, G, taken in the other line in the same manner related to the other observation; then circles being described on the two diameters DE, FG, the distance of A from one of the intersections of these circles will be the tangent of half the complement of the latitude.

Moreover, as in the treatise, which has given occasion to this discourse, it is proposed sometimes to take into consideration the ship's motion during the time, between the observations, but imperfectly, regard being had to the change made by the ship in longitude only; but the change in latitude also may be taken into consideration in the foregoing con-

(*d*) In the place above cited.

struction, by making the angle BAC equal to the sum or difference of the interval of time, and the change in the ship's longitude, according as that change is made towards the west, or towards the east, and then drawing a line from A , not to the intersection of the circles; but so, that the portions of that line, terminated by each of the circles, may be the tangents of arches, whose difference is half the change in the latitude; which, if made from the pole, requires the portion of the line terminated at the circle, whose diameter is DE , to be shorter than the other; and the contrary is required, when the motion of the ship is towards the pole.

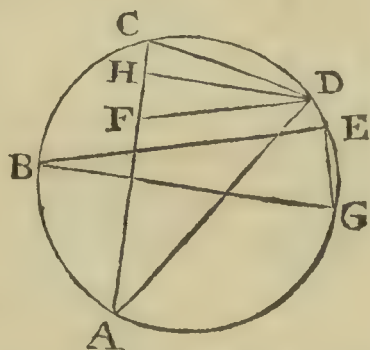
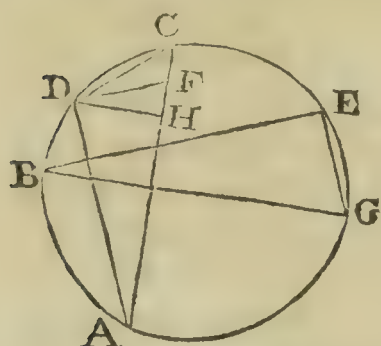
For determining the latitude by calculation, if the distance of the sun in the first observation from the zenith of the second could be found, this case would be reduced to the first, wherein the ship is considered, as stationary. And for this purpose, it has been proposed to make an additional observation, by an azimuth compass, of the angle, the ship's course makes with the azimuth of the sun, when the first altitude is taken; and, perhaps, the same angle may be found with sufficient exactness from the latitude in the first observation assumed, whether from the dead reckoning, or from the foregoing construction.

Why I have taken no notice of the calculations exhibited by the author of the piece here animadverted on, as proofs of his method, will readily appear to those, who shall cast their eye upon them.

SCHOLIUM.

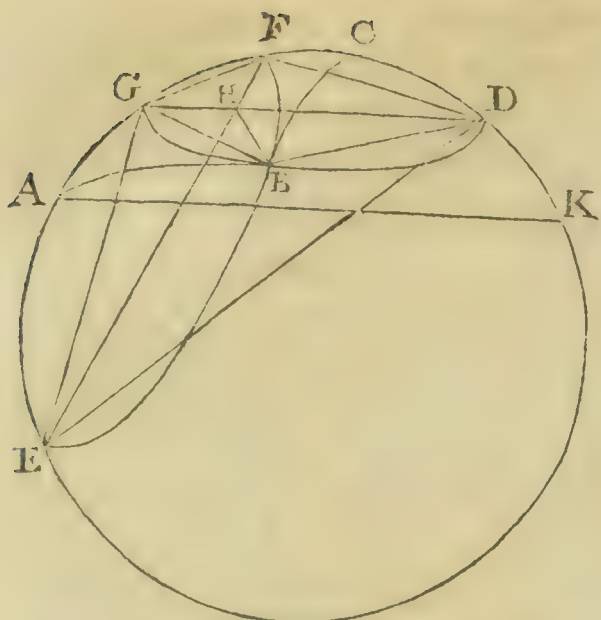
The axiom in trigonometry, on which the calculations here discoursed of have been shewn immediately

diately to depend, was introduced by Regiomontanus, and is still retained, as the foundation of the present methods of computing logarithmically an angle from the three sides of a spherical triangle given, though they may be demonstrated more directly by the following lemma.



In the circle ABC , the chord AC being drawn, and the arch ABC bisected in B , if D be taken in the circle at pleasure, and the lines BE , DF drawn also at pleasure parallel to each other, one terminated by the circle in E , and the other by the chord AC in F ; then AD , DC being drawn, $BE \times DF$ is $= AD \times DC$.

Draw DH perpendicular to AC , and also the diameter BG , joining EG . Then the angles FDH , EBG are equal, and the angles DHF , BEG right. Therefore BE is to BG as DH to DF , and $BE \times DF = BG \times DH = AD \times DC$.



Now, in the spherical triangle ABC , through B , and to the poles A, C , let the semicircles EBF, GBD be described, and the arch AC completed to a circle. Then the semicircles EBF, GBD will be both perpendi-

cular to the plane of this circle, cut it in their diameters EF, GD , and their common intersection BH be also perpendicular to it, and consequently perpendicular to GD , the diameter of the semicircle GBD , whereby GB, DB being joined, $GD \times GH$ is equal to GB^2 , and $GD \times DH$ equal to BD^2 . Again, AK being drawn parallel to GD , the arch ACK is double AC , GCD double CB , EAD the sum of all the three sides of the triangle, $EG = ED - DG$ ($2CB$), $GF = AF$ (AE) + CG (CD) - $AC = ED - 2AC$, and $FD = ED - EF$ ($2AB$).

Then EG, GF, FD, ED being joined; in the first place, AK is to DG as $AK \times GH$ to $DG \times GH$, also AK to DG as $AK \times DG$ to DG^2 , but $AK \times GH = EG \times GF$ by the preceding lemma, and $DG \times GH = GB^2$; whereby $AK \times DG$ is to DG^2 as $EG \times GF$ to GB^2 . Hence AK being twice the sine of the arch AC , DG twice the sine of the arch GC , equal to BC , EG twice the sine of half the arch EAG , GF twice

twice the sine of half the arch GF, and GB twice the sine of half the angle ACB to the radius of the circle GBD, which is half GD; $\sin. AC \times \sin. BC$ is to rad.^q as $\sin. \frac{1}{2} ED - BC \times \sin. \frac{1}{2} ED - AC$ to $\sin. \frac{1}{2} ACB]^q$. And this is Napeir's first method of finding an angle from the three sides given (*e*), as it is usually delivered.

Again, AK is to DG, or $AK \times DG$ to DG^q , as $AK \times DH$ ($ED \times DF$) to $DG \times DH$, or DB^q . Therefore, DB being twice the sine of half the angle BCD, or twice the cosine of half the angle ACB to the radius of the circle GBD, as $\sin. AC \times \sin. CB$ to rad.^q so is $\frac{1}{2} ED \times \sin. \frac{1}{2} ED - AB$ to $\cos. \frac{1}{2} ACB]^q$. And this is Napeir's second method (*f*).

And lastly, the cosine of an arch or angle being to its sine, as radius to the tangent, $\sin. \frac{1}{2} ED \times \sin. \frac{1}{2} ED - AB$ will be to $\sin. \frac{1}{2} ED - BC \times \sin. \frac{1}{2} ED - AC$ as rad.^q to $\text{tang.} \frac{1}{2} ACB]^q$, which is a third method added by Gellibrand (*g*).

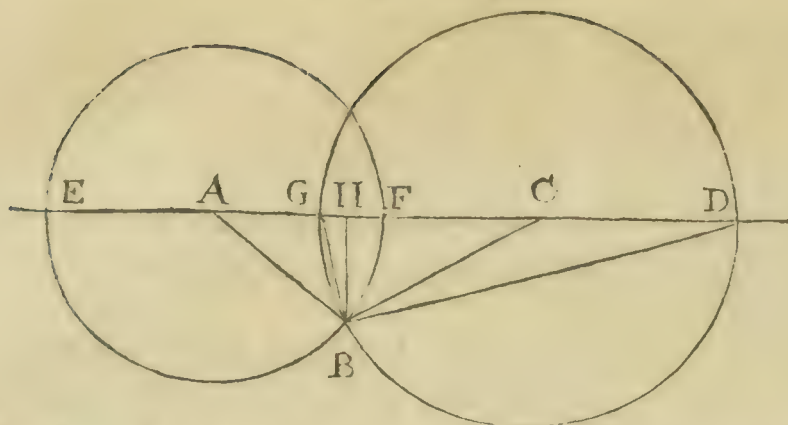
Moreover,

In plain triangles, from the three sides given may an angle be found by a process similar to each of these, as follows:

(*e*) Mirific. Canon. Log. descrip. lib. ii. c. 6. § 8.

(*f*) Ibid. § 9.

(*g*) Trigon. Britan. p. 75.



In the plain triangle ABC , if on the centers A and C be described circles through B , cutting AC in E, F, G , and D , the perpendicular BH being let fall on AC , $EH \times HF$ is equal to $GH \times HD$. Therefore $HF : GH :: HD : HE$, and by composition $GF : GH :: ED : HE$, and $:: ED - GF : EG$; also $HF : GF :: HD : ED$, and $:: DF : ED - GF$.

Moreover ED is $= AB + AC + CB$, $EG = ED - 2CB$, $FD = ED - 2AB$, $ED - GF = EG + FD = 2ED - 2CB - 2AB = 2AC$. Hence GF is to GH as $2AC$ to EG , whence $GF \times EG = 2AC \times GH$; and DH is to ED as DF to $2AC$, that is, $2AC \times DH$ will be $= ED \times DF$.

Thus $2AC : DG (:: 2AC \times DG : DG^2) :: 2AC \times GH (EG \times GF) : DGH (GB^2)$ and $AC \times CB : \text{rad.}^2 :: \frac{1}{2}ED - CB \times \frac{1}{2}ED - AC : \sin. \frac{1}{2}ACB^2$.

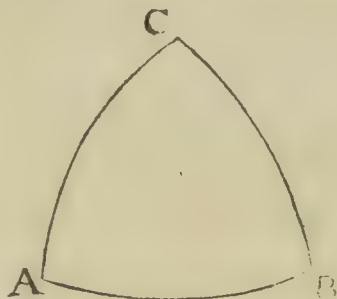
Likewise $2AC : DG (:: 2AC \times DG : DG^2) :: 2AC \times DH (ED \times DF) : GDH (DB^2)$; therefore $AC \times CB : \text{rad.}^2 :: \frac{1}{2}ED \times \frac{1}{2}ED - AB : \cos. \frac{1}{2}ACB^2$.

And in the last place $\frac{1}{2}ED \times \frac{1}{2}ED - AB : \frac{1}{2}ED - CB \times \frac{1}{2}ED - AC :: \text{rad.}^2 : \tan. \frac{1}{2}ACB^2$.

This last proposition was first delivered by Gellibrand, and the other two not long after by Oughtred, in their respective treatises of Trigonometry.

BUT it may farther here be noted, that from two sides of a plain triangle, as AC , CB , with the angle between them ACB , the third side AB may be found either by the first or second of the foregoing proportions; for by the first may be found $EG \times GF$, that is, $AF^2 - AG^2$, and by the second $ED \times DF$, or $AD^2 - AF^2$; whence by a table of squares AF ($= AB$) may be readily found, for AG is equal to $AC - BC$, and $AD = AC + CB$.

In like manner in the spherical triangle from the sides AC , CB , and the angle ACB , may the third side be expeditiously found with the aid of a table of natural sines, by the axiom referred to in the be-



ginning of this discourse, as modelled for this purpose by Naper, in the proposition, which has been already referred to (*b*), whereby the axiom is reduced to this analogy,

$$\text{Rad.}^2 : \sin. AC \times \sin. CB :: \sin. \frac{1}{2} ACB^2 \\ : \text{rad.} \times \frac{\text{verf. f. } AB - \text{verf. f. } AC \cos CB}{2} \quad (i).$$

MAY

(*b*) Mirif. Canon. Constr. p. 59.

(*i*) An example of this method has been given above in the triangle ABC in p. 920. And in default of a table of natural sines, the base AB might have been found thus. The sum of twice the logarithmic sine of $\frac{1}{2} ACB$, added to the logarithmic sines of AC and

MAY it here be farther added, that thus all the cases of oblique spherical triangles may be solved without dividing them into rectangular? For when two sides and the angle included, or when two angles and the side between them are given, the two other angles or sides may be found by the proportions of Napier and Briggs, whereby the other two angles or sides are found together (*k*); the proportions, when two sides with the including angle are given, being these,

$$\text{Cof. } \frac{AC + CB}{2} : \text{cof. } \frac{AC \oslash CB}{2} :: \text{cotang. } \frac{1}{2} ACB \\ : \text{tang. } \frac{CAB + CBA}{2}.$$

And

$$\text{Sin. } \frac{AC + CB}{2} : \text{fin. } \frac{AC \oslash CB}{2} :: \text{cotang. } \frac{1}{2} ACB \\ : \text{tang. } \frac{CAB \oslash CBA}{2}.$$

The second of these has been demonstrated with great conciseness by Dr. Halley, from the principles of the stereographic projection of the sphere (*l*); and the first is derived by him from the second, but not

and BC amounted to 37.596411. The logarithmic sine of $\frac{AC \oslash BC}{2}$ (9 562468) deducted from half this sum (18.798211)

leaves 9.235737. This number sought among the logarithmic tangents exhibits for its correspondent line 9.22940, which being deducted from the foresaid half sum (18.79211) leaves 9.568805, which is the logarithmic sine of $21^{\circ} 44' 50\frac{1}{2}''$, equal to half AB.

(*k*) Vide Neper. Mirif. Canon. Constr. pag. penult.

(*l*) See Jones's Synopsis, p. 281.

altogether

altogether with the same success, by his not observing, that if the base and one of the sides be completed to semicircles, the second of the two proportions in the supplemental triangle thence formed leads directly to the first in the original triangle.

And to conclude, if two sides, and the angle opposite to one of them, or two angles with the side opposite to one, were given; when the other opposite part is found from the proportion between the sines of parts opposite, the remaining angle, or side, may be found by either of the two proportions foregoing.

LXXXII. *An Account of the Plants Halefia and Gardenia: In a Letter from John Ellis, Esq; F. R. S. to Philip Carteret Webb, Esq; F. R. S.*

Dear Sir,

Read Nov. 20, 1760. **Y**OU must have observed, that as the spirit of planting has increased in this kingdom, the study of botany has become more fashionable; the works of the celebrated Linnæus, heretofore looked on as capricious and strange, are now in the hands of every man, who wishes to study the order of nature.

The great variety of plants, which you have introduced into your garden from North America, as well as from many other parts of the world, must give you double pleasure, when you view them ranged in proper order, and judiciously named.

The

The calling of plants after the names of botanists, as well as after the names of those, that have been, or are the promoters and encouragers of this useful science, is not without its advantages; and this custom has been followed by Linnæus, and recommended by him in his *Philosophia Botanica*. It has this good effect; it stimulates many to the searching after new discoveries, and still further improvements in agriculture and vegetation, a truly wise and laudable end.

The intent of this letter is to lay before you the characters of two new genera of plants, now growing in your garden, which I shall take the liberty to call after our worthy friends Dr. Stephen Hales of Teddington, and Dr. Alexander Garden, physician in Charlestown, South Carolina.

The plant, which I shall first describe, is the *Halesia*; and in order to give you a better idea of it, I shall transcribe the account Dr. Garden sent me of it, when he sent the specimens and seeds.

“ This beautiful tree grows commonly along the
 “ banks of Santee river, and rises often to the size
 “ of middling mulberry-trees. I have seen it some-
 “ times more southerly, near the small rills of water;
 “ but of a much smaller size than that which grows
 “ on Santee. The wood is hard, and veined; the
 “ bark is of a darkish colour, with many irregular
 “ shallow fissures. The leaves are ovated, and sharp
 “ pointed, with the middle depressed, growing al-
 “ ternately on short footstalks. The flowers hang
 “ in small bunches all along the branches, each gem
 “ producing from four to eight or nine flowers, bell-
 “ shaped, and of a pure snowy whiteness. As they
 “ blow





A. *HALESIA* /*tetraptera*/fructibus membranaceo-quadrangulatis
 B. *HALESIA* /*diptera*/fructibus alatis

“ blow early in the spring, before the leaves appear,
 “ and continue for two or three weeks, they make
 “ a most elegant appearance. They are followed
 “ by pretty large four-winged fruit, which likewise
 “ hang in bunches, each containing four kernels,
 “ that are very agreeable to the taste.”

This tree is mentioned by Catesby, Vol. I. p. 64.
 and called *Frutex padi foliis non ferratis, floribus monopetalis albis, campaniformibus, &c. &c.*

I have not heard that it was cultivated in England, till about four years ago, when the Doctor sent me over from Carolina a large parcel of the seeds, which I distributed among many curious gardeners, and others: but I cannot find any one that it has succeeded so well with as Mr. James Gordon, gardener at Mile-end, a man, who seems to be possessed of a knowledge peculiar to himself, in raising all the rarer and most difficult exotics from seeds, layers, or cuttings. He informs me, that it stands our winter in the open air, without any shelter; which confirms what Mr. Clayton of Virginia has lately wrote, that it stands even the severity of the winter of that country. So that it may, in a little time, prove a most agreeable ornament to our gardens in the spring.

About two years ago, I received from Governor Ellis of Georgia another species of this tree, which was sent him by Mr. De Brahme, from Augusta in Georgia, three hundred miles up the river Savannah.

The fruit of this kind has two wings, as described in the Plate, at B. [*Vide Tab. XXII.*]

The characters of the *Halesia* are as follows.

- CAL. (a) *Perianthium* monophyllum, superum, minimum, persistens, quadridentatum: denticulis acutiusculis, erectis.
- COR. (b) *Petalum* campanulatum, magnum, ventricosum; *limbus* quadrilobus: lobis minimis, obtusis, patulis.
- STAM. (c d) *Filamenta* (d) duodecim, (raro sedecim) tubo corollæ inserta, basi connata, subulata, erecta, corollâ paulo breviora. *Antherae* (c) oblongæ, obtusæ, erectæ.
- PIST. (e f g) *Germen* (e) oblongum, inferum. *Stylus* (f) filiformis, corollâ longior, persistens. *Stigma* (g) simplex.
- PER. (h) *Nux* corticata, sub cortice sulcata, oblonga, utrinque angustata, membranis angulata, quadrilocularis (k).
- SEM. (i) solitaria, lineari-oblonga.

The species are,

- tetraptera. 1. *HALESIA* fructibus membranaceo-quadrangulatis.
- diptera. 2. *HALESIA* fructibus alatis.

The other plant, which I am to describe, is known by the name of the Cape Jasmin, and is the most rare and beautiful shrub, that has yet been introduced into the European gardens, as well for the refreshing aromatic smell of its double milk-white flowers, as the perpetual verdure of its leaves, which are like those of the lemon-tree. [*Vide Tab. XXIII.*]

It



The Calyx of the GARDENIA with the flower not yet expanded to shew the contortion of the segments of the Petal. B. The Corolla of a single flower according to Dr. Linnaeus. C. The Tube of the Corolla cut open to shew the 5. Stamina. D. The Pistillum. E. The top of the Calyx cut off to shew the upper part of the Germen at F. — The Pericarpium cut horizontally to shew the Rudiments of the Seeds below the receptacle of the Flower at F. — H. The Corolla of the common Jasmine cut open to shew the 2 Stamina. I. The Pistillum of the Jasmine with its Germen above the receptacle of the Flower. K. The fruit of the Jasmine above the receptacle of the flower. —

St. Myrtilis vulg.



It promises, from the thickness and woodiness of its stem, together with its free manner of growing, to become a shrub of six or seven feet high.

It bears but one flower at the end of a branch; and the leaves grow opposite to each other on the branches.

We are indebted to Capt. Hutchinson, of the Godolphin Indiaman, for this curious discovery, who, about six years ago, found it growing near the cape of Good Hope, and, on his arrival here, presented it to Richard Warner, Esq; of Woodford in Essex; who finding great difficulty in propagating this valuable plant, either from cuttings or by inarching it on the yellow Indian jasmine, as he had been advised, I recommended him to try Mr. James Gordon, gardener at Mile-end; and, at the same time (August 1757), by the interest of my worthy friend Gustavus Brander, Esq; F. R. S. I procured two cuttings of it for Mr. Gordon. These, with two more, which he afterwards received, he increased to so considerable a number, that, in order to dispose of them, he advertised them for sale, at five guineas a plant; and has had such success in the sale, that, reckoning the value of the plants on hands (with a proper allowance for the falling of the price, as they become more plenty), he computes this plant will be worth at least five hundred pounds sterling to him. I mention this; because I am persuaded it must give you, and all lovers of vegetation, great pleasure to see a man thus amply and deservedly rewarded for his superior merit in this art.

Having dissected many dried as well as fresh specimens of this rare plant, I found sufficient evidence

(notwithstanding the flowers being double) to prove, that it belonged to quite another class of plants, as different from the Jasmine as the Rose is from the Peony: that the fruit was below the receptacle, instead of being above it. But, in order to be more certain, you may remember, in July 1758, I procured a specimen from Mr. Warner, for my friend Dr. Linnæus's opinion. At the same time I wrote to the professor, that if he found it to be a new genus, agreeable to the description I had sent him, that he would please to call it *Warneria*, after its worthy possessor. In his answer, he sets forth the impossibility of his being exact in determining a new genus from a double flower, agreeable to the rules he has already laid down in his *Fundamenta Botanica*. But these objections were soon after fortunately removed, by accidentally finding, among his dried oriental plants, a specimen of the same kind with a single flower, which, upon expanding it in warm water, and dissecting it, he found it to agree very nearly with the description I had sent him. But Mr. Warner refusing to have it so called, and chusing that it should still remain a Jasmine, as it is commonly called, I have thought no man more worthy, as a botanist, than our friend Dr. Garden: accordingly, the professor has agreed to adopt this new genus by the name of *Gardenia*, which he says belongs to the natural order of contorted flowers, that is, to those monopetalous flowers, whose lobes, or sections of the limb of their petals, turn all to the right hand; such as the *Nerium*, *Plumeria*, *Cerbera*, *Cameraria*, *Vinca*, &c. and that it should be placed next to the *Cerbera*.

Mr.

Mr. George Dionysius Ehret, F. R. S. has lately published a most elegant plate of this plant, by the name of, *Jasminum?* ramo uniflore pleno, petalis coriaceis, with a note of interrogation, as a quære, before the word *Jasminum*; leaving the determination, whether it is a *Jasmine* or not, to a future inquiry.

The characters of the *Gardenia* are,

[*Vide Tab. XXIII.*]

- CAL. *Perianthium* monophyllum, quinquangulare, quinquepartitum: *laciniis* ensiformibus, verticalibus, strictis, erectiusculis, persistentibus.
- COR. *Petalum* hypocrateriforme; *tubus* subcylindricus, calyce longior; *limbus* quinquepartitus, planus: *laciniis* obovatis, longitudine tubi, altero margine rectiore.
- STAM. *Filamenta* quinque, tubo corollæ adnata, in fauce brevissime libera; *Antheræ* lineares suberectæ, longitudine dimidii limbi.
- PIST. *Germen* infra receptaculum, oblongum. *Stylus* filiformis, longitudine tubi corollæ, definens extra faucem in *Stigmata* duo, magna, crassa, obtusissima.
- PER. *Bacca* ficca, oblonga, bilocularis.
- SEM. plurima, minima.

There is but one species known.

Jasminoides. 1. GARDENIA.

I am, Dear Sir,

Your most affectionate

humble servant,

London, Nov. 20, 1760.

John Ellis.

LXXXIII. *An Account of an Eclipse of the Moon, 22d November 1760, observed in Surrey-Street in the Strand, London; by James Short, M. A. F. R. S.*

	h	'	"
Read Dec. 11, 1760. P Enumbra very sensible at	7	26	0
Beginning of the eclipse at	7	39	0
Quantity of obscuration = 17' 36" at - -	8	49	0
Moon's diameter almost parallel to the ho- rizon = 3' 51" at - - - - -	8	53	0
End of the eclipse at - - - - -	10	8	0

LXXXIV. *An Account of the Case of a young Man, who had lost the Use of his Hands by cleansing Brass Wire; by Mr. Samuel More, Apothecary in Jermyn-Street. Communicated by Charles Morton, M. D. Secretary to the Royal Society.*

Read Dec. 11, 1760. **T**HE disorders, to which mechanics are unavoidably subjected by their employments, have exercised the pens of several ingenious writers; among whom I would not presume to appear, but that, I hope, the publishing the following case may be of some service: for I am certain, every one, who has been in the least conversant with the labouring people of this town, must frequently have observed their hands in a condition something

Something resembling, though perhaps not quite so bad as, that of the young man, whose disorder is the subject of the following paper.

And as there is great reason to believe, that the complaint, herein treated of, frequently happens to persons employed in the dyeing business, they will hereafter have a method of cure laid before them, which, with little expence, will probably be of great service to them. And, on that consideration only, I have been induced to submit this account of the disease to the inspection of the public.

The Case of Francis Newman, Nephew and Apprentice to Mr. Newman, Dyer, in the Maize, Southwark, who had lost the Use of his Hands by cleansing Brass Wire.

FOR the more readily explaining the following case, it is proper to premise, that, in drawing brass wire for the pin-makers, the frequent passing it through the fire, to anneal it, covers it with a crust, which it is necessary to take off, before they can make use of it; and, for this purpose, it is sent to the dyers, who, letting it lie for some time in the liquor, with which they have dyed what they call Saxon colours, (which liquor is composed of water, oil of vitriol, alum, tartar, &c.) and then throwing it forcibly three or four times against the ground, the crust is, by degrees, broken off, and the wire rendered bright, and fit for use. The gratuity given for this is generally allowed to the apprentices; and in this work Francis Newman had frequently (at his leisure hours) employed himself, till about the month of August 1759, when the cuticle on the palms of his

his hands, and the inside of his fingers, was become so hard and rigid, that he was no longer capable of doing either this, or any other business.

For relief of this disorder, he applied to the person, who attends the family in capacity of apothecary, who gave him several doses of purging physic, but without success.

He was next admitted an out-patient at St. Thomas's hospital, where he attended six weeks or two months, but without receiving any benefit.

Somebody then told him, his complaint was owing to the scurvy, (to which he had been subject) and he accordingly applied himself to several persons, who advertise remedies for curing that distemper, and, among the rest, to Mr. Ward, of whom he had some pills; and once, by mistake, took two of them for a dose, which operated so violently, that every body in the family imagined he could not survive it: however, he still continued in the same condition. And now thinking, that if he was admitted an in-patient at the hospital, he should be more likely to obtain a cure, he got himself admitted, and was there about two months longer; at the end of which time he was discharged, but in no better condition than before.

About a fortnight after this, and a twelvemonth from the beginning of his disorder, *viz.* August 10, 1760, the person, who is foreman to Mr. Newman, desired leave to write to me, for my opinion of the case; which being very readily granted, he desired me, by letter, to come and see a young man, who, as he expressed it "had poisoned his hands with brass and oil of vitriol."

When

When I first visited him, I found him with his hands quite stiff, and utterly incapable of any business whatever; and having already had so much advice, and taken so many medicines, he concluded his disorder was incurable, and that he should entirely lose the use of his hands, the *skin on the palms* of them (the right hand rather the worst of the two) having the *exact appearance of parchment, full of chaps*; and when I endeavoured, by force, to streighten the fingers, the *blood started from every joint* of them.

After hearing the best account I could get of the cause of his complaint, I imagined, that, as the disease had been contracted by his frequently dipping his hands into a violently-acid liquor, the most probable method of relieving him would be, by the application of an emollient liniment, mixed with an alkaline lixivium. For this purpose, I ordered as follows:

℞ Ol. Olivar. ℥iv.

Lixivii. Salis Alkalini. fix. ℥ii. M.

F. Linimentum.

With this he was ordered to anoint his hands frequently, especially going to bed; and, to prevent the liniment being too soon rubbed off, constantly to wear a pair of gloves.

About four days after, I found the skin a little softened, and I could extend the fingers with less pain than before; and no blood issued, upon my endeavouring to move them. This would have encouraged me to have continued the use of the same liniment; but as he complained much of its making his hands

smart every time he used it, (and, indeed, this was the first application among the many he had tried, that ever gave him any uneasiness) I concluded, that the addition of some yolk of egg might lessen the acrimony of the alkaline salt, without at all abating the efficacy of the liniment: I therefore composed the liniment thus:

℞ Ol. Olivar. ℥iv.

Lixivii. Salis Alk. fix. ℥ii.

Vitel. Ovor. N^o. ii. f. Linimentum;

to be used as before. This mixture not giving him so much pain as the former, he had used it all in three days; and then, coming to me for more, I found his hands still continue to mend; the skin that had grown hard scaling off, and a new flexible one appearing underneath; the chaps were many of them healed; and he began to have some use of his fingers. Encouraged by this success, he continued the use of the last prescribed liniment; and as, from his not having had the proper use of his fingers for so long a time, the joints of them had, in a great degree, lost their motion, I advised him, alternately to clench his fist, and to stretch out his fingers, many times a day.

The disorder had been so long upon him, and (if I may be allowed the expression) had taken so deep root, that, although he began very sensibly to amend, from the first application of the liniment, yet it was full two months before I thought it adviseable to leave off the use of it; and then, to prevent a relapse, I gave him the following ointment:

℞ Axung. Porcin. ℥ii.

Vitel. Ovi.

Ol. Lavend. gr. v. f. Unguentum.

with

with orders to anoint his hands with it every night going to bed. This ointment he has continued to use about a month; and is now perfectly restored to the use of his hands, and begins again to work at his business.

During this course of anointing, he took no internal medicines, except three doses of purging physic.

LXXXV. *A further Account of some Experiments made on the Bovey Coal* *.

Read Jan. 8, 1761. **S**ALT of hartshorn mixed with the phlegm that distilled first from the Bovey coal, produced no ebullition, nor air bubbles; but when mixed with the watry liquor, which arose with the thick oil in the latter part of the process, after it had stood some weeks in a glass bottle, close stopt, and was become perfectly fine, caused a very considerable ebullition, and the mixture immediately grew foul and red. In some days after, it grew much thicker, and had the colour of tar. The surface of it was covered with a bituminous pellicle, as were the sides and bottom of the glass. Eighteen grains of salt of hartshorn were not more than sufficient to saturate the acid salts contained in an ounce of the liquor, which was but very little sour to the taste.

Spirit of nitre dropped into this bituminous liquor, soon after it was distilled, and before it had deposited the oily particles (which rendered it cloudy), changed

* See before, p. 534.

its colour to a deep brown; but had not that effect after the liquor was become transparent.

The black gritty powder, which remained after the former process, was put into a coated retort, and distilled by a naked fire; so that the whole body of the retort continued red-hot for more than two hours. This brought over to the receiver near an ounce of a watry bituminous liquor, rather stronger than that which distilled with a sand heat, and a few drops only of a thick bituminous matter, which stuck to that part of the receiver on which they fell. The neck of the retort was thinly incrustated with something that resembled a saline concrete; but was found to be only bituminous matter. In the bottom of the retort there remained a very black gritty powder.

Of this powder, one ounce was put into a crucible, set in a melting furnace, and kept in a pretty strong fire for an hour. The powder, after it was grown cold, appeared on the surface to be of a pale reddish colour; but was not in the least altered underneath. It lost, however, in weight, near three drachms. Some of the black powder taken out of the crucible, and thrown upon a red-hot iron, burnt without flame; but emitted plentifully a heavy black smoke.

Two ounces of the black powder, which had been twice distilled, were set on a clear fire, in an iron ladle, and continually stirred from the time that the ladle grew red-hot, and the matter began to emit a heavy black stinking smoke, till no more smoke arose from it. The calcined matter remaining in the ladle weighed two scruples, and seemed to be a kind of bole earth. This earth was evaporated in two ounces of rain water to one ounce, which, some days after, was poured off clean from the sediment. This water
had

had not the least saline taste, nor did it give any sign of effervescence, when spirit of nitre was dropped into it.

From the preceding experiments it appears, that the substance called fossil-wood consists, for the most part, of water, and that a considerable quantity of this principle is separated from it by a gentle heat; which seems to be the reason, why such fossil-wood, on being exposed to the sun and air, or kept in a dry place, soon becomes full of superficial cracks, resembling a piece of timber, which, by lying long on the ground in the open air, has begun to decay: that though the fossil-wood does not, like amber, or pit-coal, yield, by distillation, a light oil floating on its phlegm, and a volatile acid salt in a concrete form, yet that a light oil, and a volatile acid salt, in a considerable quantity, are intimately mixed with the water, which distils from it: that this fossil-wood differs, in several particulars, from all wood belonging to the vegetable kingdom, which has been examined by fire after the same manner.

1st, Its powder burnt in a close vessel, and kept red-hot for a much longer time, than is sufficient to reduce the like quantity of vegetable charcoal, emits (when sprinkled on a red-hot iron) a thick heavy black smoke.

2dly, The same powder, burnt as before-mentioned, doth not easily take fire, nor burn of itself, nor consume to ashes, even when exposed, in an open crucible, to a strong fire, and kept in it ignited, and almost white, for a considerable time.

3dly, The matter left by this powder, after its phlogistic principle has been separated from it by time and air, contains no alkaline salt, and appears

to be an astringent mineral earth; whereas charcoal easily takes fire, burns freely without smoke, and continues burning, till it consumes to an ash; which consists of an alkaline salt, and a pure earth, fit for making cuppels; and, by these marks, is sufficiently distinguished from all mineral substances.

Grosvenor-Street, Jan. 8, 1761.

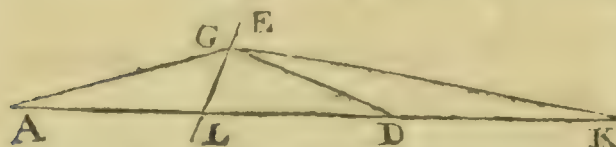
Jer. Milles.

LXXXVI. *De Aberratione Luminis, in Superficiebus et Lentibus Sphæricis refractorum* *.

§ 1.

Read April 2,
1761.

SI radius luminis AG , incidens in superficiem quamcunque refringentem LE , inflectatur secundum rectam GK , et quævis recta linea AK occurrat radio incidenti AG in A , refracto GK in K ; et rectæ GD , normali ad superficiem refringentem LE , in D ; erit rectangulum $GK \times DA$ ad rectangulum $GA \times DK$, ut sinus anguli incidentiæ DGA ad sinum anguli refractionis DGK . Est enim DA ad GA , ut $\sin. DGA$ ad $\sin. ADG$, et $GK : DK :: \sin. GDK : \sin. DGK$; quare, ob



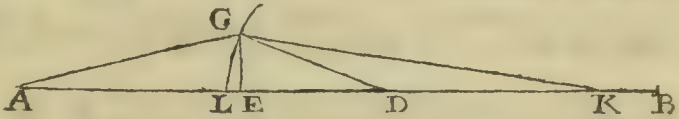
* This Paper, though sent to England in the summer of the year 1760, was, by accident, prevented from being read to the Royal Society, till the 2d of April following.

fin. $ADG = \text{fin. } KDG$, erit componendo $DA \times GK : GA \times DK :: \text{fin. } DGA : \text{fin. } DGK$. Quæ ratio, si ponatur ut i ad r , erit $i \cdot GA \times DK = r \cdot GK \times DA$.

§ 2.

Incidat radius luminis AG in superficiem sphaericam LG ,

cujus cen-

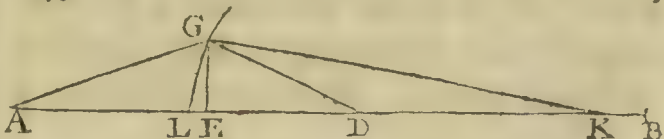
trum est D ,  et refringatur secundum rectum GK , quæritur concursus K radii refracti GK cum axe sphaeræ ALD , posito arcu LG satis parvo.

A puncto incidentiæ G cadat GE normalis ad axem AD , ducaturque radius sphaeræ GD . His factis est per elementa, $AGq = ADq + DGq - 2AD \times DE = ADq + DLq - 2AD \times DL - LE = \overline{AD - DL}q + 2AD \times LE = ALq + 2AD \times LE$, adeoque $AG = \sqrt{ALq + 2AD \times LE} = (\text{ob } LE \text{ satis parvam}) AL + \frac{AD \times LE}{AL}$ quamproxime. Similiter $KGq = KDq + DGq + 2KD \times DE = KDq + DLq + 2KD \times DL - LE = \overline{KD + DL}q - 2KD \times LE = KLq - 2KD \times LE$, adeoque $KG = \sqrt{KLq - 2KD \times LE} = KL - \frac{KD \times LE}{KL}$ quamproxime.

Jam vero est (§ 1.) $i \cdot GA \times DK = r \cdot GK \times DA$, quare substi-

tutis valori-

bus modo in-

ventis ipsarum GA et GK , habetur $i \cdot KD \times (AL + \frac{AD \times LE}{AL}) = r \cdot AD \times (KL - \frac{KD \times LE}{KL})$, 

transf-

transponendo $r . AD \times KL - i . KD \times AL = AD$
 $\times KD \times LE \times \left(\frac{i}{AL} + \frac{r}{KL} \right).$

Ponatur jam $LA = A$, $LK = K$, radius sphaeræ
 $DL = a$, adeoque $AD = A + a$, et $DK = K - a$.
 His in novissima æquatione scriptis, erit $rK \times \overline{A + a}$
 $- iA \times \overline{K - a} = \overline{A + a} \times \overline{K - a} \times LE \times \left(\frac{i}{A} + \frac{r}{K} \right),$

unde transponendo et dividendo, $K = \frac{iAa}{i - r . A - ra}$
 $- \frac{\overline{A + a} \times \overline{K - a} \times LE}{i - r . A - ra} \times \left(\frac{i}{A} + \frac{r}{K} \right).$

Jam si radius incidens AG fuerit axi AD vi-
 cinissimus, evanescet LE , adeoque et terminus
 $\frac{\overline{A + a} \times \overline{K - a} \times LE}{i - r . A - ra} \times \left(\frac{i}{A} + \frac{r}{K} \right).$ Quare in hoc casu

$K = \frac{iAa}{i - r . A - ra}$, id est, distantia foci geometrici

ipfi A conjugati à vertice L , erit $\frac{iAa}{i - r . A - ra}$, unde

aberratio radii refracti GK ab hoc foco erit
 $\frac{\overline{A + a} \times \overline{K - a} \times LE}{i - r . A - ra} \times \left(\frac{i}{A} + \frac{r}{K} \right)$, fumendo à foco

contra directionem cursus radiorum. Quoniam vero

aberratio illa semper est valde parva, erit $\frac{iAa}{i - r . A - ra}$

valor prope verus distantiae LK five K , adeoque in
 expressione aberrationis sine sensibili errore pro K
 usurpari potest. Sit itaque distantia LB foci geome-

trici B à vertice superficiei L , five $\frac{iAa}{i - r . A - ra} = B$,

eritque

eritque K five $LB = B - \frac{A + a \cdot B - a \cdot LE}{i - r \cdot A - r a} \times \left(\frac{i}{A} + \frac{r}{K} \right)$
quamproxime.

Dimidia latitudo superficiei refringentis, five distantia puncti incidentiæ G ab axe, id est LG, dicatur L, erit quamproxime $LE = \frac{L^2}{2a}$: quare in formula modo inventa pro LE scribendo $\frac{L^2}{2a}$, et ulterius concinnando expressionem aberrationis ope æquationis $B = \frac{i A a}{i - r \cdot A - r a}$, habetur formula, qua in sequentibus præcipue utemur: $LK = B - \frac{r \cdot i - r \cdot B^2 L^2}{2 i^3} \times \left(\frac{1}{a} + \frac{1}{A} \right)^2 \times \left(\frac{r}{a} + \frac{i + r}{A} \right)$.

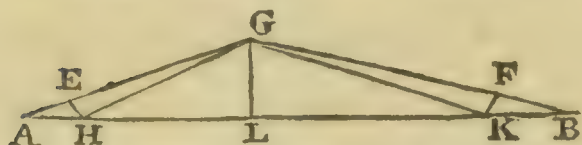
§ 3.

In formulis jam inventis signa symbolorum illi tantum casui sunt accommodata, quem figura exprimit, ubi radius luminis ab axe divergens ponitur incidere in superficiem convexam, et post refractionem ad axem convergere. Accommodantur vero ad reliquos problematis casus, mutando signum radii sphaeræ a , si radius luminis incidat in superficiem concavam, et signum ipsius A , si radius incidens convergat ad axem. Hoc facto, si valor distantiae LB five B prodit positivus, sumenda est distantia illa à vertice L secundum directionem cursus radiorum; si negativus, contra eandem directionem. Aberratio vero radii à foco B, five BK per formulas computata, si fuerit positiva, sumenda est à foco B contra directionem cursus radiorum; si negativa, secundum hanc directionem. Si radii incidentes fuerint axi pa-

ralleli, facienda est A infinita, et si B prodit infinita, radii refracti erunt axi paralleli, abstrahendo ab illorum aberratione.

§ 4.

Si duo radii luminis AG , HG , in angulo quam minimo AGH



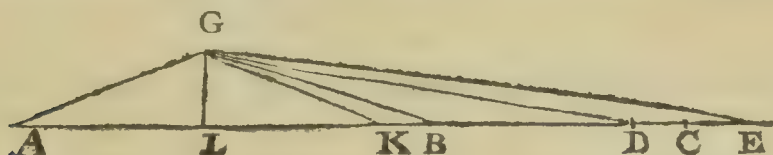
inter se inclinati incidant fere perpendiculariter in

idem punctum G superfici ei cujuscunque refringentis LG , et refringantur, prior in GK , posterior vero in GB , occurrentes lineæ rectæ cuicunque in punctis A , H , K , B ; dico esse BK ad AH ut est $r \cdot LBq$ ad $i \cdot LAq$, posita i ad r ut sinus incidentiæ ad sinum refractionis.

Quoniam enim anguli valde parvi sunt sinibus suis quamproxime proportionales, erunt in parvis refractionibus anguli incidentiæ et refractionis eorumque adeo differentiæ sinibus incidentiæ et refractionis proportionales quamproxime. Est itaque angulus AGH , utpote differentia angulorum incidentiæ radiorum AG , HG , ad angulum BGK , siue differentiam angulorum refractionis radiorum GK , GB , ut i ad r . Quare centro G inter hos angulos descriptis arcubus HE , KF , erit HD ad KF , ut $i \cdot GH$ ad $r \cdot GK$; sed est $AH = \frac{GH \cdot HE}{GL}$, et $BK = \frac{GK \cdot KF}{GL}$, quare pro HE et KF , scribendo earum proportionales $i \times GH$ et $r \times GK$, habetur AH ad BK ut $i \times GHq$ ad $r \times GKq$, quamproxime, id est, ut $i \times LAq$ ad $r \times LBq$.

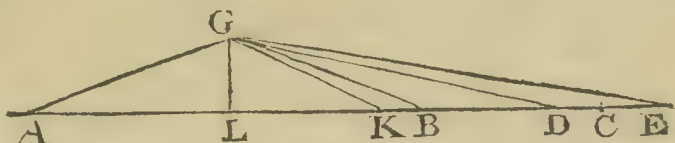
§ 5.

Data forma lentis refringentis, et data in ejus axe distantia foci radiorum incidentium, invenire punctum concursus radii cujusvis refracti cum axe.



Sit locus lentis in L , ejus axis $ALBC$, dimidia latitudo LG , five distantia incidentiæ ab axe, $= L$; radius convexitatis superficiæ primæ, in quam radii incidunt, $= a$; radius convexitatis superficiæ secundæ, è qua emergunt, $= b$, radius luminis quivis incidens AG , refractus GK . In axe lentis ABC sit distantia foci radiorum incidentium, five $LA = A$, distantia foci geometrici ipsi A conjugati, five $LB = B$; distantia foci geometrici C superficiæ primæ ipsi A conjugati, five $LC = C$, et evidens est, esse etiam idem punctum C focus geometricum superficiæ secundæ conjugatum ipsi B . Sit tandem distantia puncti concursus K radii refracti GK cum axe, five $LK = K$.

Radius AG in prima superficie refractus tendat versus punctum D , et concipiatur à puncto B , quod est focus lentis ipsi A conjugatis, radius BG incidere



in secundam superficiem, et in ea ita refringi, ut divergat à puncto axeos E . Et dabuntur per § 2. aberrationes CE et CD , adeoque earum summa DE . Sed per § 4. est DE ad BK ut $r.LC$ q ad $i.LB$ q,

quare $BK = \frac{i \cdot LBq}{r \cdot LCq} \times DE = \frac{iB^2}{rC^2} \times DE$. Jam per

§ 2. est $CD = \frac{r \cdot \overline{i-r} \cdot C^2 L^2}{2i^3} \times \left(\frac{1}{a} + \frac{1}{A}\right) \times \left(\frac{r}{a} + \frac{i+r}{A}\right)$

et $CE = \frac{r \cdot \overline{i-r} \cdot C^2 L^2}{2i^3} \times \left(\frac{1}{b} + \frac{1}{B}\right) \times \left(\frac{r}{b} + \frac{i+r}{B}\right)$,

quarum summa DE , ducta in $\frac{i \cdot B^2}{r \cdot C^2}$, dat aberrationem

quæsitam $BK = \frac{iB^2}{rC^2} \times DE = \frac{\overline{i-r} \cdot B^2 L^2}{2i^2} \left[\left(\frac{1}{a} + \frac{1}{A}\right)^2 \times \left(\frac{r}{a} + \frac{i+r}{A}\right) + \left(\frac{1}{b} + \frac{1}{B}\right)^2 \times \left(\frac{r}{b} + \frac{i+r}{B}\right) \right]$, quæ

post evolutionem terminorum subtracta de LB , five B , relinquit distantiam puncti concursus $LK =$

$$B - \frac{\overline{i-r} \cdot B^2 L^2}{2i^2} \times \left\{ \frac{r}{a^3} + \frac{i+3r}{Aa^2} + \frac{2i+3r}{A^2a} + \frac{i+r}{A^3} + \frac{r}{b^3} + \frac{i+3r}{Bb^2} + \frac{2i+3r}{B^2b} + \frac{i+r}{B^3} \right\}.$$

Porro quoniam punctum C est focus ipsi A conjugatus respectu faciei primæ, erit $LC = \frac{iAa}{\overline{i-r} \cdot A - ra}$;

et quoniam idem punctum C est focus ipsi B conjugatus respectu superficiei secundæ, erit $LC =$

$$- \frac{iBb}{\overline{i-r} \cdot B - rb}, \text{ per § 2. Hinc } \frac{iAa}{\overline{i-r} \cdot A - ra} =$$

$$- \frac{iBb}{\overline{i-r} \cdot B - rb}, \text{ adeoque } B = \frac{rAab}{\overline{i-r} \cdot A \cdot a + b - rab},$$

five $\frac{1}{A} + \frac{1}{B} = \frac{i-r}{r} \times \frac{1}{a} + \frac{1}{b}$. Dato sic valore ipsius

B , cognoscitur distantia concursus radii refracti GK cum axe five LK , ejusque aberratio à foco B five BK , quæ ob signum — sumenda est à foco B contra directionem

directionem cursus radiorum, si valorem habet positivum; secundum vero, si negativum.

Accommodantur vero hæ formulæ ad reliquos problematis casus eadem ratione ac dictum est § 3. de aberratione superficiei simplicis.

§ 6.

Expressio aberrationis jam inventa, ad formam reducitur usui magis accommodatam, conjungendo terminos, in quibus a et b easdem habent dimensiones, et reducendo singulas partes ope æquationis $\frac{i-r}{r} \times \frac{1}{a} + \frac{1}{b}$.

$= \frac{1}{A} + \frac{1}{B} = \frac{1}{P}$, ubi P est distantia foci principalis.

Hac ratione calculum subducendo habetur: $\frac{1}{a^3} + \frac{1}{b^3}$

$$= \frac{r}{i-r.P} \times \left(\frac{r^2}{(i-r)^2.P^2} - \frac{3r}{i-r.P.(a+b)} \right); \frac{1}{A^2 a} + \frac{1}{B^2 b}$$

$$= \frac{r}{i-r.P} \times \left(\frac{1}{A a} + \frac{1}{B b} - \frac{1}{P.(a+b)} \right); \frac{1}{A^2 a} + \frac{1}{B^2 b} =$$

$$\frac{r}{i-r.P} \times \left(\frac{i-r}{r} \times \frac{1}{A a} + \frac{1}{B b} - \frac{1}{P.(A+B)} \right); \frac{1}{A^3} + \frac{1}{B^3}$$

$$= \frac{r}{i-r.P} \times \left(\frac{i-r}{r P^2} - \frac{3.(i-r)}{r P.(A+B)} \right).$$

Singulæ harum partium ductæ in suos respective coëfficientes, r , $i+3r$, $2i+r$, $i+r$, conjungantur cum signis propriis, et habebitur: $LK = LB - BK =$

$$B - \frac{B^2 L^2}{2 i P^2} \left(\frac{i^3 - 2 i^2 r + 2 r^3}{(i-r)^2.P} - \frac{3 i + 2 r}{A+B} + \frac{2.(i+r)}{A+B} \times \frac{A}{b} + \frac{B}{a} - \frac{r.(i+2r)}{i-r.(a+b)} \right), \text{ vel etiam } = B - \frac{B^2 L^2}{2 i P^2} \times \left(\frac{i^3}{(i-r)^2.P} - \frac{3 i + 2 r}{A+B} - \frac{2.(i+r)}{A+B} \times \frac{A}{a} + \frac{B}{b} - \frac{r.(i+2r)}{i-r.(a+b)} \right).$$

§ 7.

§ 7.

Invenire lentem, quæ radios luminis à data distantia advenientis in alia data distantia colligat cum minima aberratione, ipsamque aberrationem minimam.

Retentis omnibus symbolis ut supra, sumantur differentialia aberrationis modo inventæ, $\frac{B^2 L^2}{2i \cdot P^2} \times \left(\frac{i^3}{i-r \cdot P} \right.$

$$\left. - \frac{3i+2r}{A+B} - \frac{2 \cdot \overline{i+r}}{A+B} \times \frac{\overline{A}}{a} + \frac{\overline{B}}{b} - \frac{r \cdot \overline{i+2r}}{i-r \cdot \overline{a+b}} \right),$$

positis a et b variabilibus, et habebitur $\frac{2 \cdot \overline{i+r}}{A+B}$

$$\times \frac{\overline{A da}}{a^2} \times \frac{\overline{B db}}{b^2} + \frac{r \cdot \overline{i+2r}}{i-r} \times \frac{da+db}{a+b^2} = 0; \text{ fed ob}$$

$$\frac{1}{a} + \frac{1}{b} = \frac{r}{i-r \cdot P}, \text{ est } \frac{da}{a^2} + \frac{db}{b^2} = 0; \text{ quare tollendo}$$

per comparationem harum æquationum differentialia,

$$\text{invenitur: } \frac{b-a}{b+a} = \frac{2 \cdot \overline{i+r} \cdot \overline{i-r}}{r \cdot \overline{i+2r}} \times \frac{A-B}{A+B}, \text{ adeoque}$$

$$\frac{a}{b} = \frac{4r^2 + ri - 2i^2 \cdot A + i \cdot 2i+r \cdot B}{4r^2 + ri - 2i^2 \cdot B + i \cdot 2i+r \cdot A}; \text{ unde per æqua-}$$

$$\text{tionem } \frac{1}{a} + \frac{1}{b} = \frac{r}{i-r} \times \frac{1}{A} + \frac{1}{B}, \text{ colliguntur radii facierum}$$

$$\text{lentis quæsitæ, scil. } a = \frac{2 \cdot \overline{i-r} \cdot \overline{i+2r} \cdot AB}{i \cdot 2i+r \cdot A + 4r^2 + ri - 2i^2 \cdot B},$$

$$\text{et } b = \frac{2 \cdot \overline{i-r} \cdot \overline{i+2r} \cdot AB}{i \cdot 2i+r \cdot B + 4r^2 + ri - 2i^2 \cdot A}; \text{ five } \frac{1}{a} =$$

$$\frac{i \cdot 2i+r}{2 \cdot \overline{i-r} \cdot \overline{i+2r}} \times \frac{1}{B} + \frac{4r^2 + ri - 2i^2}{2 \cdot \overline{i-r} \cdot \overline{i+2r}} \times \frac{1}{A}; \text{ et } \frac{1}{b} =$$

$$\frac{i \cdot 2i+r}{2 \cdot \overline{i-r} \cdot \overline{i+2r}} \times \frac{1}{A} + \frac{4r^2 + ri - 2i^2}{2 \cdot \overline{i-r} \cdot \overline{i+2r}} \times \frac{1}{B}.$$

Hic

Hic valores radiorum a et b substituti in formula aberrationis generali § 6. dant ipsam aberrationem

$$\text{minimam} = \frac{i \cdot B^2 L^2}{2 \cdot i + 2r \cdot P^2} \times \left(\frac{P}{A + B} + \frac{r \cdot \overline{4i - r}}{4 \cdot i - r^2} \right).$$

§ 8.

Postquam in § præcedente formam lentis invenimus, quæ in datis circumstantiis aberrationem minimam producat, et simul ipsam ejus aberrationem; rationi consentaneum est, investigationem aberrationis lentium ab hac forma discendentium ita aggredi, ut simul appareat relatio formarum harum lentium ad illius formam, et aberrationis harum ad illius aberrationem. Solent enim conclusiones prodire concinniores, si ad casum inter reliquos maxime singularem et unicum referantur, qualis hic est casus aberrationis minimæ.

Scribatur brevitatis causa b pro $\frac{i \cdot \overline{2i + r}}{2 \cdot i - r \cdot i + 2r}$, et K pro $\frac{4r^2 + ri - 2i^2}{2 \cdot i - r \cdot i + 2r}$, et erit pro determinandis radiis a et b lentis aberrationis minimæ, $\frac{1}{a} = \frac{b}{B} + \frac{K}{A}$, et $\frac{1}{b} = \frac{b}{A} + \frac{K}{B}$, per § 7. Ponatur jam generaliter pro radiis a et b facierum cujuscvis alius lentis, $\frac{1}{a} = \frac{b}{B} + \frac{K}{A} + \frac{x}{P}$, et $\frac{1}{b} = \frac{b}{A} + \frac{K}{B} - \frac{x}{P}$; quos valores ipsarum $\frac{1}{a}$ et $\frac{1}{b}$ tales assumo, ut earum summa $\frac{1}{a} + \frac{1}{b}$ fit = $\frac{b}{A} + K \times \frac{1}{A} + \frac{1}{B}$, sive $\frac{r}{i - r} \times \frac{1}{A} + \frac{1}{B}$, quemadmodum

dum oportet. Substituantur hi valores ipsarum $\frac{1}{a}$ et $\frac{1}{b}$ in formula aberrationis generali, $\frac{B^2 L^2}{2 i P^2} \times \left(\frac{i^3}{P} - \frac{3i + 2r}{A + B} - \frac{2 \cdot \overline{i + r}}{A + B} \times \frac{\overline{A}}{a} + \frac{\overline{B}}{b} - \frac{r \cdot \overline{i + 2r}}{i - r \cdot \overline{a + b}} \right)$ § 6. et peracto calculo invenietur $\frac{i B^2 L^2}{2 \cdot \overline{i + 2r} \cdot P^3} \left(\frac{P}{A + B} + \frac{\overline{4i - r \cdot r}}{4 \cdot \overline{i - r}^2} \cdot \frac{\overline{i + 2r}^2}{i^2} x^2 \right)$ pro aberratione lentis, cujus facies luminis obversa radium habeat a , et facies à lumine averfa radium habeat b , existentibus $\frac{1}{a} = \frac{b}{B} + \frac{K}{A} + \frac{x}{P}$, et $\frac{1}{b} = \frac{b}{A} + \frac{K}{B} - \frac{x}{P}$; P distantia foci principalis, A distantia foci radiorum incidentium, et B refractorum.

Commodum itaque hic accidit, ut numerus x , qui relationem formæ lentis ad lentem aberrationis minimæ designat, idem quoque relationem aberrationis ejus ad aberrationem minimam ratione non minus simplici exhibeat.

§ 9.

Si in formulis § præced. exhibitis, pro radiis facierum lentis ejusque aberratione, statuatur $x = 0$, radit lens ad eam formam, quæ aberrationem dat minimam, et aberratio ejus ad aberrationem minimam. Si vero non fuerit $x = 0$, apparet ex iisdem formulis, cuivis aberrationis quantitati, quæ major sit aberratione minima, duas diversas respondere lentis formas, quarum utraque definitur æquationibus $\frac{1}{a} = \frac{b}{B} + \frac{K}{A} + \frac{x}{P}$, et $\frac{1}{a} = \frac{b}{A} + \frac{K}{B} - \frac{x}{P}$, ubi x pro altera

altera positive, altera negative accipitur. Nam in formula aberrationis $\frac{i B^2 L^2}{2 \cdot i + 2r \cdot P^3} \times \left(\frac{P}{A+B} + \frac{\overline{4i-r \cdot r}}{4 \cdot \overline{i-r^2}} + \frac{\overline{i+2r^2}}{i^2} x^2 \right)$ non comparet nisi quadratum ipsius x , quod idem manet, five radix x fumatur positive, five negative.

Quoniam aberratio quævis $\frac{i B^2 L^2}{2 \cdot i + 2r \cdot P^3} \times \left(\frac{P}{A+B} + \frac{\overline{4i-r \cdot r}}{4 \cdot \overline{i-r^2}} + \frac{\overline{i+2r^2}}{i^2} x^2 \right)$ est ad aberrationem minimam $\frac{i B^2 L^2}{2 \cdot i + 2r \cdot P^3} \times \left(\frac{P}{A+B} + \frac{\overline{4i-r \cdot r}}{4 \cdot \overline{i-r^2}} \right)$ in ratione majoris inæquabilitatis, statuatur illa ratio $1 + m^2$ ad 1 ; assumendo m pro numero quovis, et invenitur $x = \pm \frac{mi}{i+2r} \sqrt{\left(\frac{P}{A+B} + \frac{\overline{4i-r \cdot r}}{4 \cdot \overline{i-r^2}} \right)}$. Et sic ex data ratione aberrationis alicujus lentis ad aberrationem minimam, datur illi correspondens numerus x , adeoque et radii facierum ejus, per superiora.

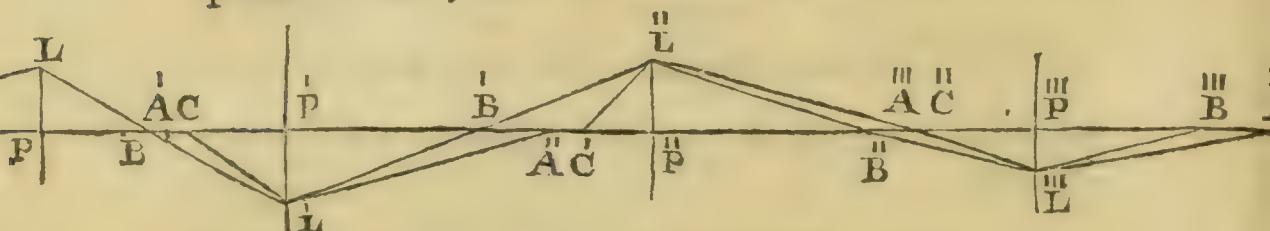
Numerus hic x , quoniam simul et formam et aberrationem suæ lentis indicat, dicatur *index* lentis, et scribatur brevitatis causa f pro $\frac{\overline{4i-r \cdot r}}{4 \cdot \overline{i-r^2}}$, et g pro $\frac{i+2r}{i}$, ut aberratio sit $\frac{B^2 L^2}{2g \cdot P^3} \times \left(\frac{P}{A+B} + f + g^2 x^2 \right)$.

§ 10.

In linea recta A O, tanquam axe communi, dispositæ intelligantur lentes quocunque P, P^I, P^{II}, P^{III}, &c. quarum foci conjugati sint respective, lentis

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primæ A et $\overset{I}{A}$; secundæ $\overset{I}{A}$ et $\overset{II}{A}$; tertiæ $\overset{II}{A}$ et $\overset{III}{A}$;
quartæ $\overset{III}{A}$ et $\overset{IV}{A}$, &c.



Radius luminis, veniens à puncto A, refringatur in omnibus his lentibus in punctis L, $\overset{I}{L}$, $\overset{II}{L}$, $\overset{III}{L}$, &c. pergens secundum cursum A L $\overset{I}{L}$ $\overset{II}{L}$ $\overset{III}{L}$, et post singulas refractiones occurrat axi A O in punctis B, $\overset{I}{B}$, $\overset{II}{B}$, $\overset{III}{B}$, &c. aberrans à focis $\overset{I}{A}$, $\overset{II}{A}$, $\overset{III}{A}$, $\overset{IV}{A}$, &c. longitudinibus $\overset{I}{A}B$, $\overset{II}{A}B$, $\overset{III}{A}B$, $\overset{IV}{A}B$, &c. Quæruntur jam hæ aberrationes.

Lentium P, $\overset{I}{P}$, $\overset{II}{P}$, $\overset{III}{P}$, &c. distantiae focorum principalium dicantur respective P, $\overset{I}{P}$, $\overset{II}{P}$, $\overset{III}{P}$, &c. et indices κ , $\overset{I}{\kappa}$, $\overset{II}{\kappa}$, $\overset{III}{\kappa}$, &c. Distantiae focorum conjugatorum à suis respective lentibus sint: pro lente prima P, $PA = A$, $P\overset{I}{A} = B$; pro lente secunda $\overset{I}{P}$, $\overset{I}{P}A = \overset{I}{A}$, $\overset{I}{P}\overset{I}{A} = \overset{I}{B}$; pro lente tertia $\overset{II}{P}$, $\overset{II}{P}A = \overset{II}{A}$, $\overset{II}{P}\overset{II}{A} = \overset{II}{B}$; pro lente quarta $\overset{III}{P}$, $\overset{III}{P}A = \overset{III}{A}$, et $\overset{III}{P}\overset{III}{A} = \overset{III}{B}$, et ita porro: ponatur præterea PL, sive distantia puncti incidentiæ in primam lentem ab axe = L, et erit $\overset{I}{P}L = \frac{\overset{I}{A}}{\overset{I}{B}} \times L$, $\overset{II}{P}L = \frac{\overset{II}{A} \cdot \overset{I}{A}}{\overset{II}{B} \cdot \overset{I}{B}} \times L$, $\overset{III}{P}L = \frac{\overset{III}{A} \cdot \overset{II}{A} \cdot \overset{I}{A}}{\overset{III}{B} \cdot \overset{II}{B} \cdot \overset{I}{B}} \times L$, et ita porro.

I. Vidimus in § 9. aberrationem $\overset{I}{A}B$ lentis primæ P esse $\frac{B^2 L^2}{2g \cdot P^3} \times \left(\frac{P}{\overset{I}{A} + \overset{I}{B}} + f + g^2 x^2 \right)$, ubi $f = \frac{4i - r \cdot r}{4 \cdot i - r^2}$, et $g = \frac{i + 2r}{i}$.

II. Concipiatur à foco secundæ lentis $\overset{II}{A}$ ipsi $\overset{I}{A}$ conjugato in illam incidere radius $\overset{II}{A}\overset{I}{L}$, qui refractus secundum $\overset{I}{L}C$, conveniat cum axe in C , aberrans à foco $\overset{I}{A}$ longitudine $\overset{I}{A}C$: et erit per § 9. aberratio $\overset{I}{A}C = \frac{\overset{I}{A}^4 L^2}{2g \cdot B^2 \cdot P^3} \times \left(\frac{\overset{I}{P}}{\overset{I}{A} + \overset{I}{B}} + f + g^2 x^2 \right)$, scribendo scilicet $\frac{\overset{I}{A}}{B} \times L$ pro L , $\overset{I}{B}$ pro A , $\overset{I}{A}$ pro B , et $\overset{I}{P}$ pro P . Hæc aberratio $\overset{I}{A}C$, addita ad aberrationem primæ lentis modo inventam, $\overset{I}{A}B$, dat summam BC . Est vero BC ad $\overset{II}{B}\overset{II}{A}$ ut $\overset{I}{P}\overset{I}{A}q$ ad $\overset{II}{P}\overset{II}{A}q$, sive $\overset{I}{A}^2$ ad $\overset{II}{B}^2$, id est quantitates aberrationum à focis conjugatis sunt ut quadrata distantiarum focorum à lente, quod facile probari potest. Ergo aberratio $\overset{II}{A}\overset{I}{B}$, à refractione per duas lentes P et $\overset{I}{P}$ producta, habetur $= \frac{\overset{II}{B}^2}{\overset{I}{A}^2} \times BC$

$$= \frac{\overset{II}{B}^2 L^2}{2g} \times \left[\frac{B^2}{\overset{I}{A}^2 \cdot P^3} \left(\frac{P}{\overset{I}{A} + \overset{I}{B}} + f + g^2 x^2 \right) + \frac{\overset{I}{A}^2}{B^2 \cdot P^3} \left(\frac{\overset{I}{P}}{\overset{I}{A} + \overset{I}{B}} + f + g^2 x^2 \right) \right].$$

III. Concipiatur fimiliter à foco interiori A^{III} lentis
tertiæ P^{II} incidere in illam radium $A^{III} L^{II}$, cumque re-
fractum convenire cum axe in puncto C^I , et aberrare
à foco exteriori A^{II} longitudine $A^{II} C^I$, quæ per § 9. est

$\frac{A^I \cdot A^{II} \cdot L^2}{2g \cdot B^2 \cdot B^{II} \cdot P^3} \times \left(\frac{P^{II}}{A^{II} + B^{II}} + f + g^2 x^2 \right)$. Hæc addita
ad aberrationem duarum priorum lentium modo in-

ventam, $A^{II} B^I$, dat summam $A^{II} B^I + A^{II} C^I = B^I C^I$, de-
inde fiat ut A^{II}^2 ad B^{II}^2 ; ita hæc summa $B^I C^I$ ad aber-

rationem trium lentium $A^{III} B^{II} = \frac{B^{II} \cdot L^2}{2g} \times \left[\frac{B^2 \cdot B^I}{A^2 \cdot A^{II} \cdot P^3} \right.$
 $\left(\frac{P}{A + B} + f + g^2 x^2 \right) + \frac{B^2 \cdot A^2}{B^2 \cdot A^{II} \cdot P^3} \left(\frac{P^{II}}{A^{II} + B^{II}} + f + g^2 x^2 \right)$
 $\left. + \frac{A^2 \cdot A^{II}}{B^2 \cdot B^{II} \cdot P^3} \left(\frac{P^{II}}{A^{II} + B^{II}} + f + g^2 x^2 \right) \right]$.

IV. Eadem ratione invenitur aberratio $A^{IV} B^{III}$ post
refractionem per quatuor lentes $P, P^I, P^{II}, P^{III} =$

$\frac{B^{III} \cdot L^2}{2g} \times \left[\frac{B^2 \cdot B^I \cdot B^{II}}{A^2 \cdot A^{II} \cdot A^{III} \cdot P^3} \times \left(\frac{P}{A + B} + f + g^2 x^2 \right) \right.$
 $+ \frac{B^2 \cdot B^{II} \cdot A^2}{B^2 \cdot A^{II} \cdot A^{III} \cdot P^3} \times \left(\frac{P^{II}}{A^{II} + B^{II}} + f + g^2 x^2 \right) + \frac{B^2 \cdot A^2 \cdot A^{II}}{B^2 \cdot B^{II} \cdot A^{III} \cdot P^3}$
 $\times \left(\frac{P^{II}}{A^{II} + B^{II}} + f + g^2 x^2 \right) + \frac{A^2 \cdot A^{II} \cdot A^{III}}{B^2 \cdot B^{II} \cdot B^{III} \cdot P^3} \left(\frac{P^{III}}{A^{III} + B^{III}} \right.$
 $\left. + f + g^2 x^2 \right) \left. \right]$. Et eadem ratione progredi licet ad
definiendam

definiendam aberrationem radiorum post transitum per plures lentes.

In his computationibus omnes lentes concipiuntur convexæ, et foci radiorum incidentium ponuntur jacerere ante lentes suas, refractorum vero post lentes respectu cursus radiorum, quemadmodum figura exhibet. Formulæ autem allatæ ad quosvis alios casus facile accommodantur, mutando signa symbolorum, prout requirit mutatio hypotheseos, quod monuisse sufficiat.

§ II.

Datis positionibus et focorum principalium distantis lentium quocunque, oporteat invenire formas earum, quæ efficiant, ut radii luminis, à dato in communi earum axe puncto advenientes, post refractionem colligantur in ultimo foco sine aberratione, si fieri potest.

Solutio problematis generaliter absolvitur, ponendo expressionem aberrationis, datæ multitudini lentium convenientem, (§ 10.) nihilo æqualem, quo ipso habetur æquatio definiens rationem requisitam indicum singularum lentium x , x^I , x^{II} , x^{III} , &c. Et quoniam relatio omnium indicum unica hac æquatione continetur, patet multum hic locum esse arbitrariæ nonnullorum assumptioni, prout requirunt vel suadent aperturæ lentium aliæque circumstantiæ ex scopo instrumenti dijudicandæ, de quibus hic non est sermo. In genere tantum cavendum est, ne assumptio ita fiat, ut aliquis indicum prodeat imaginarius.

Determinatis sic indicibus x , x^I , x^{II} , x^{III} , &c. inveniuntur radii facierum per § 8. Dicatur scilicet radius faciei

faciei anterioris singularum lentium a , posterioris b ; distantia foci principalis à lente, P ; distantia foci radiorum incidentium ante lentem, A ; distantia foci radiorum refractorum post lentem, B ; et index ad lentem pertinens, x ; et erit $\frac{1}{a} = \frac{b}{B} + \frac{K}{A} + \frac{x}{P}$, et $\frac{1}{b} = \frac{b}{A} + \frac{K}{B} - \frac{x}{P}$, ubi $b = \frac{i \cdot 2i + r}{2 \cdot i - r \cdot i + 2r}$, et $K = \frac{4r^2 + ri - 2i^2}{2 \cdot i - r \cdot i + 2r}$, unde dabuntur singularum lentium radii a et b .

Si nullus detur valor realis indicum x , x^I , x^{II} , x^{III} , &c. id quod accidit, si omnes æquationis termini ad unam partem rejecti fuerint ejusdem signi, calus propositus est impossibilis.

§ 12.

EXEMPLUM I. Ex duabus lentibus telescopium componere, quod sit liberum ab aberratione, et objecta visa amplifcet in data ratione n ad 1. Abstrahatur autem hic a reliquis boni telescopii requisitis.

Lentes duæ primæ P et $\overset{I}{P}$ repræsentent binas lentes telescopii quæsitæ, P quidem objectivam et $\overset{I}{P}$ ocularem (vid. fig. § 10.). Aberratio harum duarum lentium (§ 10. cas. 11.) nihilo æqualis posita dat æquationem generalem: $g^2 \times \left(\frac{B^2 \cdot x^2}{A^2 \cdot P^3} + \frac{\overset{I}{A}^2 \cdot \overset{I}{x}^2}{B^2 \cdot \overset{I}{P}^3} \right) + f \times \left(\frac{B^2}{A^2 \cdot P^3} + \frac{\overset{I}{A}^2}{B^2 \cdot \overset{I}{P}^3} \right) + \frac{B^2}{A^2 \cdot P^2 \cdot A + B} + \frac{\overset{I}{A}^2}{B^2 \cdot \overset{I}{P}^2 \cdot \overset{I}{A} + \overset{I}{B}} = 0$. Quoniam per constitutionem telescopii, radii luminis

minis in lentem objectivam incidentes, et è lente oculari emergentes, sunt axi telescopii paralleli, erit A infinita, adeoque $B = P$; item $\overset{I}{B}$ infinita, adeoque $\overset{I}{A} = \overset{I}{P}$. Quare deletis duobus ultimis æquationis terminis, utpote evanescentibus, et in reliquis in scribendo P et $\overset{I}{P}$ pro B et $\overset{I}{A}$, habetur pro casu hujus exempli æquationis $g^2 \times (P x^2 + \overset{I}{P} x^2) + f \times (P + \overset{I}{P}) = 0$, relationem indicum x et $\overset{I}{x}$ definiens, requisitam ad id, ut aberratio radiorum è lente oculari emergentium nulla sit. Jam si in hac æquatione P et $\overset{I}{P}$ ponantur ejusdem signi, sive utraque lens convexa, vel utraque concava, nullus valor realis haberi potest pro x et $\overset{I}{x}$, adeoque problema in hoc casu erit impossibile. Statuatur itaque $\overset{I}{P}$ negativa, sive lens ocularis concava, sumendo pro exponente amplificationis objecti telescopio visi, n , numerum quemvis positivum, et faciendo $\overset{I}{P} = -\frac{1}{n} P$, ut patet ex opticis. Scripto hoc valore pro $\overset{I}{P}$ in æquatione indicum, evadit illa $g^2 \times (x^2 - n x^2) = f \times n - 1$. Horum itaque indicum utrumlibet pro lubitu assumere licet, modo ne $\overset{I}{x}$ fiat minor, quam $\frac{\sqrt{n-1} \cdot f}{g}$, quod redderet indicem x imaginarium. Altero indicum sic assumpto, alter determinatur per allatam æquationem $g^2 \times (x^2 - n x^2) = f \cdot n - 1$. Quo facto radii facierum utriusque lentis prodeunt per §. II. ut sequitur:

Radius faciei anterioris lentis objectivæ = $\frac{P}{b+x}$,
 posterioris = $\frac{P}{K-x}$;

Radius faciei anterioris lentis ocularis = $\frac{\overset{I}{P}}{K+x}$,
 posterioris = $\frac{\overset{I}{P}}{b-x}$, ubi $\overset{I}{P}$ sumenda est negativa, sive
 $\overset{I}{P} = -\frac{I}{n}P$, ut dictum est.

Pro quovis itaque pari valorum correspondentium indicum x et $\overset{I}{x}$, quatuor prodeunt diversæ problematis solutiones, ob signa duplicia ipsarum x et $\overset{I}{x}$, unde duæ prodeunt formæ utriusque lentis, quarum quælibet combinari potest cum binis formis alterius lentis.

Si detur vel assumatur forma alterutrius lentis, forma alterius per supradicta commode determinatur. Sit v. gr. lens objectiva plano-convexa, habens faciem convexam antrorsum versam, planam retrorsum. Radius faciei ejus posterioris $\frac{P}{K-x}$ statuatur infinitus, et erit $x = K$, adeoque radius faciei, anterioris $\frac{P}{b+x} = \frac{P}{b+K} = \frac{i-r}{r} \times P$. Pro x scribatur K in æquatione indicum $\overset{I}{x}^2 - nx^2 = \frac{n-1}{g^2} \cdot f$, et erit $\overset{I}{x} = \sqrt{\frac{n-1}{g^2} \cdot f + nK^2}$, unde dantur radii facierum lentis ocularis, idque dupliciter, ob signum duplex valoris ipsius $\overset{I}{x}$.

Vidimus

Vidimus in solutione hujus problematis, aberrationem à duabus lentibus productam non posse evanescere, si utraque fuerit convexa, vel utraque concava, cujuscumque sint formæ et quomodocunque componentur. Idem quoque obtinet in quacunque lentium multitudine, quæ omnes sint convexæ, vel omnes concavæ. Omnes enim lentes, quarum facies superficierum sphæricarum sunt segmenta, aberrationes a focus suis eo producunt, quod radios luminis refractione nimium inflectunt; unde facile perspicitur, lentem convexam, convexis additam, vel concavam concavis, errores a prioribus productos augere. Quare ut aberratio a foco ultimo evanescat, debent lentium aliæ esse convexæ, aliæ concavæ, quo nimix radiorum incurvationes in unam partem corrigantur per nimias incurvationes factas in partem contrariam.

§ 13.

EXEMPLUM II. Propositum sit investigare formas binarum lentium, quæ juxta se positæ radios incidentes axi parallelos in data à lentibus distantia R colligant, citra aberrationem ex figura facierum sphærica oriundam.

Quoniam in casu hujus exempli radii incidentes ponuntur paralleli erit in æquatione (§ 10. cas. 2.) pro relatione binorum indicum x et x^i , A infinita, adeoque $B = P$. Et quoniam lentes ponuntur juxta se positæ, erit earum distantia $B + A = 0$, sive $A = -B = -P$. Præterea est etiam $\frac{1}{B} = \frac{1}{P} + \frac{1}{P}$,

unde $\frac{1}{B} - P = -\frac{P^2}{P + P}$. Substitutis itaque his

valoribus, erit æquatio pro relatione indicum in casu præfenti: $g^2 \cdot (\dot{P}^3 \cdot x^2 + P^3 \cdot \dot{x}^2) + f \cdot P^3 + \dot{P}^3 = P \cdot \dot{P} \cdot P + \dot{P}$, five $P^3 \dot{x}^2 + \dot{P}^3 x^2 = \frac{P + \dot{P}}{g^2} \times (\overline{1 + f} \cdot P \dot{P} - f \cdot P^2 + \dot{P}^2)$, quæ assumpto numero quovis n , et statuendo $\dot{P} = nP$, evadit $x^2 + n^3 x^2 = \frac{n + 1}{g^2} \times (n - \overline{n^2 - n + 1} \cdot f)$. Ne autem casus propositus fiat impossibilis, ob utramque lentem convexam, vel utramque concavam, per n semper intelligi debet numerus negativus. Assumatur jam pro Cubitu alteruter indicum x et $\dot{x}$, et dabitur alter per æquationem allatam.

Determinato vero utroque indice, radii facierum erunt ut sequitur:

Radius faciei anterioris lentis primæ = $\frac{P}{b + x}$,
posterioris = $\frac{P}{K - x}$;

Radius faciei anterioris lentis secundæ = $\frac{\dot{P}}{n + 1 \cdot b - nK + x}$, posterioris = $\frac{\dot{P}}{n + 1 \cdot K - nb - x}$,

ubi est $nP = \dot{P}$, posito n numero negativo.

Distantia foci principalis systematis binarum lentium P et $\dot{P}$ dicatur R , et quoniam est $\frac{1}{R} = \frac{1}{P} + \frac{1}{\dot{P}}$, erit $P = \frac{n + 1}{n} R$, et $\dot{P} = \overline{n + 1} \cdot R$, quare in expressionibus radiorum scribendo hos valores pro P et $\dot{P}$, habebuntur:

$$\text{Radius faciei anterioris lentis primæ} = \frac{\frac{n+1}{n} R}{b+x},$$

$$\text{posterioris} = \frac{\frac{n+1}{n} R}{K-x};$$

$$\text{Radius faciei anterioris lentis secundæ} = \frac{\frac{n+1}{n+1} \cdot R}{n+1 \cdot b - nK+x}, \text{ posterioris} = \frac{\frac{n+1}{n+1} \cdot R}{n+1 \cdot K - nb-x}.$$

Et hæ lentès juxta se positæ radios incidentes axi parallelòs in distantia à lente, R , colligent sine aberratione, si distantia foci lentis concavæ major fuerit distantia foci lentis convexæ; si autem contra dispergentur radii sine aberratione à puncto ante lentem, cujus distantia positivè sumpta est R .

Pro casu singulari hujus exempli, ponamus requiri formas binarum lentium ita comparatarum, ut distantia foci principalis lentis concavæ sit ad distantiam foci principalis lentis convexæ, ut 3 ad 2, et ut radii colligantur in distantia R à lentibus. Si lens anterior debet esse convexa, erit $n = -\frac{3}{2}$, et æquatio definiens relationem indicum lentis convexæ x et lentis concavæ x , $8x^2 - 27x^2 = \frac{19f+6}{g^2}$. De-

terminatis vero decenter indicibus x et x , radii facierum erunt, ut sequitur:

$$\text{Radius faciei anterioris lentis primæ} = \frac{R}{3b+3x},$$

$$\text{posterioris} = \frac{R}{3K-3x};$$

$$\text{Radius faciei anterioris lentis secundæ} = \frac{R}{3K-b+2x}, \text{ posterioris} = \frac{R}{3b-K-2x}.$$

Si vero lens anterior debet esse concava, erit $n = -\frac{2}{3}$, et relatio indicum $8x^2 - 27x^2 = \frac{19f+6}{g^2}$, radii vero facierum ut sequitur:

$$\text{Radius faciei anterioris lentis primæ} = -\frac{R}{2b + 2x^2},$$

$$\text{posterioris} = -\frac{R}{2K - 2x^2};$$

$$\text{Radius faciei anterioris lentis secundæ} = \frac{R}{b + 2K + 3x^2},$$

$$\text{posterioris} = \frac{R}{K + 2b - 3x^2}.$$

§ 14.

EXEMPLUM III. Propositum sit, telescopium ex tribus lentibus componere, quarum prima P et secunda P^I juxta se positæ vitrum objectivum duplex constituent, cujus distantia foci principalis sit R ; tertia vero five ocularis P^{II} , habeat distantiam foci $P^{II} = \frac{R}{m}$, ut potentia amplificandi designetur per numerum m . Ponatur præterea $P^I = nP$, intelligendo per n numerum datum negativum, eumque unitate majorem, si lens convexa anterior est collocanda, minorem si posterior, ut radii post refractionem in vitro objectivo convergant, quemadmodum expositum est in exemplo præcedente. Quærentur autem formæ harum trium lentium, ut telescopium liberum sit ab omni erratione ex figura illarum sphaerica oriunda.

Aberratio à tribus lentibus P, P^I, P^{II} , oriunda (§ 10. cas. III.) nihilo æqualis posita, dat $\frac{B^2 \cdot B^2}{A^2 \cdot A^2 \cdot P^3} \left(\frac{P}{A + B} \right)$

†

$$+ f + g^2 x^2) + \frac{\overset{I}{B}^2 \cdot \overset{I}{A}^2}{\overset{I}{B}^2 \cdot \overset{II}{A}^2 \cdot \overset{I}{P}^3} \left(\frac{\overset{I}{P}}{\overset{I}{A} + \overset{I}{B}} + f + g^2 \overset{I}{x}^2 \right) \\ + \frac{\overset{I}{A}^2 \cdot \overset{II}{A}^2}{\overset{I}{B}^2 \cdot \overset{I}{B}^2 \cdot \overset{II}{P}} \left(\frac{\overset{II}{P}}{\overset{II}{A} + \overset{II}{B}} + f + g^2 \overset{II}{x}^2 \right) = 0.$$

Symbola hanc æquationem ingredientia per hypothesin hujus exempli ita determinantur: ob A infinitam est $\frac{P}{A+B} = 0$, et $B = P$. Ob $\overset{II}{B}$ infinitam

est $\frac{\overset{II}{P}}{\overset{II}{A} + \overset{II}{B}} = 0$, et $\overset{II}{A} = \overset{II}{P}$. Ob lentes P et $\overset{I}{P}$ juxta

se positas; est earum distantia $\overset{I}{A} + B = 0$, five $\overset{I}{A} = -B = -P$. Ob $\frac{\overset{I}{R}}{\overset{I}{P}} = \frac{\overset{I}{P}}{\overset{I}{P}} + \frac{\overset{I}{P}}{\overset{I}{P}}$, et $\overset{I}{P} = nP$, est

$P = \frac{n+1}{n}R$, et $\overset{I}{P} = \frac{n+1}{n}R$. Præterea est $\overset{I}{B} = R$,

et $\overset{II}{P} = \frac{R}{m}$. His itaque valoribus substitutis, et æqua-

tione ordinata, habetur relatio indicum $x, \overset{I}{x}, \overset{II}{x}$, expressa ut sequitur: $g^2 x \cdot (n^3 x^2 + \overset{I}{x}^2 + \frac{n+1}{m} \overset{II}{x}^2)$

$+ f \times (n^3 + 1 + \frac{n+1}{m}) = n \cdot \frac{n+1}{n}$. Determinatis indicibus $x, \overset{I}{x}, \overset{II}{x}$, habentur per § 11. radii

facierum: videlicet,

Radius faciei anterioris lentis primæ = $\frac{n+1}{n} \cdot \frac{R}{b+x}$,

posterioris = $\frac{n+1}{n} \cdot \frac{R}{K-x}$;

Radius faciei anterioris lentis secundæ = $\frac{n+1}{n+1} \cdot \frac{R}{b-nK+x}$, posterioris = $\frac{n+1}{n+1} \cdot \frac{R}{K-nb-x}$;
Radius

$$\begin{aligned} \text{Radius faciei anterioris lentis tertiæ} &= \frac{-R}{m \cdot K + \frac{11}{x}}, \\ \text{posterioris} &= \frac{R}{m \cdot b - \frac{11}{x}}. \end{aligned}$$

Ita si vitrum objectivum hujus telescopii componendum sit ex duabus lentibus, convexa et concava, quarum distantiae focales sint ut 3 et 2, debeatque primo lens convexa anterior collocari, oportebit sumere $n = -\frac{3}{2}$, et erit æquatio pro indicibus:

$$g^2 \times \left(27x^2 - 8x^2 + \frac{11x^2}{m} \right) + f \times \left(19 + \frac{1}{m} \right) + 6 = 0, \text{ et radii facierum:}$$

$$\begin{aligned} \text{Radius faciei anterioris lentis primæ} &= \frac{R}{3b + 3x}, \\ \text{posterioris} &= \frac{R}{3K + 3x}; \end{aligned}$$

$$\begin{aligned} \text{Radius faciei anterioris lentis secundæ} &= \\ -\frac{R}{3K - b + 2x}, \text{ posterioris} &= -\frac{R}{3b - K - 2x}; \end{aligned}$$

$$\begin{aligned} \text{Radius faciei anterioris lentis tertiæ} &= \frac{R}{mK + \frac{11}{x}}, \\ \text{posterioris} &= \frac{R}{mb - \frac{11}{x}}. \end{aligned}$$

Si vero lens concava anterior est collocanda, debet sumi $n = -\frac{2}{3}$, unde æquatio pro indicibus prodit

$$g^2 \times \left(-8x^2 + 27x^2 + \frac{11x^2}{m} \right) + f \times \left(19 + \frac{1}{m} \right) + 6 = 0, \text{ et consequenter:}$$

$$\begin{aligned} \text{Radius faciei anterioris lentis primæ} &= -\frac{R}{2b + 2x}, \\ \text{posterioris} &= -\frac{R}{2K - 2x}; \end{aligned}$$

Radius

$$\text{Radius faciei anterioris lentis secundæ} = \frac{R}{b + 2K + 3x}, \text{ posterioris} = \frac{R}{K + 2b - 3x};$$

$$\text{Radius faciei anterioris lentis tertiæ} = \frac{R}{mK + m\alpha},$$

$$\text{posterioris} = \frac{R}{mb - m\alpha}.$$

§ 15.

Vidimus in solutione generati novissimi problematis (§ ii.) et subjunctis exemplis (§ 12, 13, 14.) relationem indicum x , x' , x'' , &c. unica tantum æquatione definiri, adeoque judicio artificis plurimum esse relictum in commoda eorum assumptione facienda. Duæ autem sunt considerationes, quibus hæc assumptio utcunque dirigi possit. Prima est, ut nullus indicum fiat nimis magnus, five, quod eodem recidit, ut formæ lentium quamproxime accedant ad eas, quæ in datis circumstantiis aberrationem dant omnium minimam. Hac enim ratione errores in forma lentium forte commissi minus nocebunt, quemadmodum ex natura minimi notum est. Altera est, ut singulæ lentes, quam fieri potest, proxime evadant æqualiter utrinque convexæ, vel æqualiter concavæ, quo fiet, ut majores aperturæ sint patientes. Quantitates enim aberrationum supra definitæ non sunt nisi quamproxime veræ, et tanto magis fallunt, quanto facies lentium fuerint majora suarum sphærarum segmenta. Binæ hæ regulæ, etiamsi sibi invicem sæpius adversuntur, nec facile definiri queat, quantum in quovis dato casu uni vel alteri ipsarum sit tribuendum; non dubito tamen, quin judicioso artifice sint profuturæ

profuturæ ad mediam viam tenendam inter utramque.

Eædem quoque considerationes impediunt, quominus certi aliquid definire auserim, de maximo possibili effectu instrumentorum opticorum secundum hæc principia compositorum, vel de modo perfectissima componendi. Regulæ huc pertinentes tutissime expectantur ab experientia, modo dirigatur à theoria.

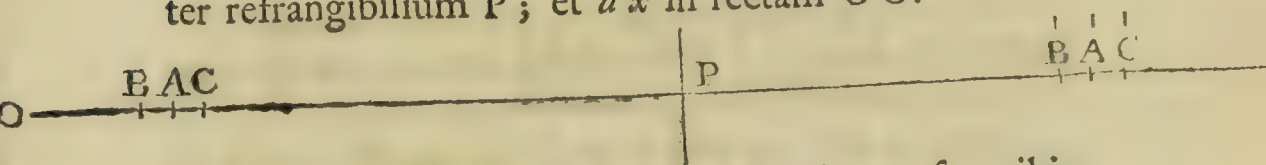
§ 16.

Expositis jam breviter quæ dicenda habuimus de aberrationibus radiorum homogeneorum refractorum, ortis a lentium figura sphærica, ut et de modo illas corrigendi in instrumentis opticis: liceat etiam pauca addere de altera illa aberrationis specie, quæ a diversa radiorum refrangibilitate oritur, quæ objecta trans lentes visa coloribus inficit, et maximam confusionis partem producit. Creditum hucusque communiter fuit hoc vitium nulla arte emendabile, donec ingeniosissimus artifex Londinensis, Dollandus, experimentis institutis feliciter deprehenderet, varia dari vitrorum genera, quæ, licet fere æqualiter refringant certum quoddam radiorum genus, plurimum tamen discrepent viribus refringendi reliqua radiorum genera, adeoque et radios heterogeneous a se invicem separandi: quo eximio invento et theoriam et praxim optices insigniter auxit. Discimus enim hinc, rationes refractionem radiorum diversi generis nullo modo a se invicem dependere, ut hucusque fuit creditum; adeoque frustra quæri regulam, qua ex datis refractionibus radiorum diversi generis in uno aliquo medio inveniantur refractiones eorundem in alio;
vel

vel qua ex data refractione unius alicujus radii in quovis medio, inveniuntur refractiones reliquorum in eodem medio. Praxis quoque telescopiorum dioptricarum insigne incrementum hujus inventi beneficio nacta est. Scilicet sagacissimus inventor modum inde derivavit corrigendi aberrationes ex separatione radiorum heterogeneorum oriundas, componendo vitrum objectivum ex duabus lentibus, una convexa, altera concava, quæ ita sint comparatæ, ut una alterius effectum in radiis separandis destruat, quemadmodum in Vol. L. Part. II. p. 733. et seq. Transactionum Philosophicarum exposuit. Quod artificium ita breviter explicabimus.

§ 17.

Sit P lens quæcunque, habens radios facierum suarum a et b ; distantiam foci principalis mediocriter refrangibilium P ; et $a \propto$ in rectam OO .



Ratio refractionis radiorum mediocriter refrangibilium ex aëre in lentem sit N ad 1 ; radiorum maxime refrangibilium $N + n$ ad 1 , et radiorum minime refrangibilium $N - n$ ad 1 , existente n numero valde parvo respectu ipsius N . In axe lentis OO sint puncta A, B, C foci radiorum in lentem P incidentium, A mediocriter refrangibilium, B maxime et C minime refrangibilium, quorum focorum distantiae AB, AC sint valde parvæ, et ponantur puncta $\overset{1}{A}, \overset{1}{B}, \overset{1}{C}$, foci conjugati ipsorum A, B, C respective, pro sua quique radiorum specie, ita ut sint $BC, \overset{1}{B}\overset{1}{C}$,
 VOL. LI. 61 dissipa-

diffipationes radiorum heterogeneousorum circa focos conjugatos A et $\overset{I}{A}$. Quærat^r earum relatio.

Quoniam B et $\overset{I}{B}$ sunt foci conjugati radiorum maxime refrangibilium, quorum ratio refractionis est $N + n$ ad 1, erit $\frac{1}{PB} + \frac{1}{P\overset{I}{B}} =$

$\overline{N + n - 1} \cdot \frac{1}{a} + \frac{1}{b}$. Similiter quoniam puncta C et $\overset{I}{C}$

sunt foci conjugati radiorum minime refrangibilium, quorum ratio refractionis est $N - n$ ad 1, erit

$\frac{1}{PC} + \frac{1}{P\overset{I}{C}} = \overline{N - n + 1} \cdot \frac{1}{a} + \frac{1}{b}$. Subtrahatur hæc

posterior æquatio à priore, et habebitur $\frac{1}{PB} - \frac{1}{P\overset{I}{C}}$

$+ \frac{1}{P\overset{I}{B}} - \frac{1}{P\overset{I}{C}} = 2n \cdot \frac{1}{a} + \frac{1}{b}$, five conjungendo binos

terminos: $\frac{PC - PB}{PB \times PC} + \frac{P\overset{I}{C} - P\overset{I}{B}}{P\overset{I}{B} \times P\overset{I}{C}} = 2n \cdot \frac{1}{a} + \frac{1}{b}$, quæ

pro $PB \times PC$ et $P\overset{I}{B} \times P\overset{I}{C}$, scribendo PAq et

$P\overset{I}{A}q$, et pro $\frac{1}{a} + \frac{1}{b}$ scribendo $\frac{1}{N - 1 \cdot P}$, evadit $\frac{B\overset{I}{C}}{P\overset{I}{A}q}$

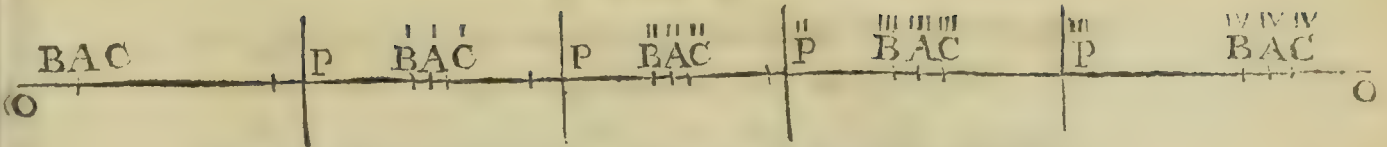
$= \frac{2n}{N - 1 \cdot P} + \frac{BC}{PAq}$, exhibens relationem inter dif-

fipationes conjugatas BC et $B\overset{I}{C}$.

His præmissis, intelligantur lentes quotcunque P, $\overset{I}{P}$,

$\overset{II}{P}$, $\overset{III}{P}$, &c. quarum distantie focorum principalium respectu radiorum mediocriter refrangibilium sint

P, $\overset{I}{P}$, $\overset{II}{P}$, $\overset{III}{P}$, &c. respective, ordine dispositæ in axe
carum



earum communi OO . Rationes refractionum radiorum mediocriter refrangibilium in his lentibus ordine sumtis sint respective, N ad I , N^I ad I , N^{II} ad I , N^{III} ad I , &c. radiorum maxime refrangibilium $N + n$ ad I , $N^I + n^I$ ad I , $N^{II} + n^{II}$ ad I , $N^{III} + n^{III}$ ad I , &c. et radiorum minime refrangibilium $N - n$ ad I , $N^I - n^I$ ad I , $N^{II} - n^{II}$ ad I , $N^{III} - n^{III}$ ad I , &c. Puncta A , A^I , A^{II} , A^{III} , A^{IV} , &c. sint foci conjugati lentium respectu radiorum mediocriter refrangibilium, puncta B , B^I , B^{II} , B^{III} , B^{IV} , &c. respectu radiorum maxime refrangibilium, C , C^I , C^{II} , C^{III} , C^{IV} , &c. respectu minime refrangibilium, ita ut BC , $B^I C^I$, $B^{II} C^{II}$, $B^{III} C^{III}$, $B^{IV} C^{IV}$, &c. sint dissipationes successivæ radiorum heterogeneorum in axe lentium. Dicantur tandem distantiae focorum conjugatorum à suis respective lentibus: $PA = A$, $PA^I = B$; $PA^I = A$, $PA^{II} = B$; $PA^{II} = A$, $PA^{III} = B$; $PA^{III} = A$, $PA^{IV} = B$, &c.

Jam si in æquatione pro relatione dissipationum conjugatarum modo inventa, $\frac{B^I C^I}{PA^I q} = \frac{2n}{N - I \cdot P} + \frac{BC}{PA q}$,

per BC et $B^I C^I$ intelligantur successive singula paria dissipationum conjugatorum, et per PA et PA^I distantiae focorum conjugatorum ipsis respondentes;

item per P, N et n eorum valores singulis lentis proprii, habebuntur tot æquationes, quot sunt lentes:

$$\text{videlicet; } \overset{I}{B}\overset{I}{C} = B^2 \times \left(\frac{2n}{\overset{I}{N} - \overset{I}{I} \cdot \overset{I}{P}} + \frac{BC}{A^2} \right); \overset{II}{B}\overset{II}{C} = \overset{I}{B}^2 \times \left(\frac{2n}{\overset{II}{N} - \overset{II}{I} \cdot \overset{II}{P}} + \frac{\overset{I}{B}\overset{I}{C}}{A^2} \right); \overset{III}{B}\overset{III}{C} = \overset{II}{B}^2 \times \left(\frac{2n}{\overset{III}{N} - \overset{III}{I} \cdot \overset{III}{P}} + \frac{\overset{II}{B}\overset{II}{C}}{A^2} \right) + \frac{\overset{II}{B}\overset{II}{C}}{A^2}; \overset{IV}{B}\overset{IV}{C} = \overset{III}{B}^2 \times \left(\frac{2n}{\overset{IV}{N} - \overset{IV}{I} \cdot \overset{IV}{P}} + \frac{\overset{III}{B}\overset{III}{C}}{A^2} \right); \text{ et ita}$$

porro, si plures fuerint lentes. Ponantur puncta B, A, C, coincidere, five ex primo foco A prodire radium compositum, ut dissipatio prima BC fit nulla, et reducendo has æquationes, ponendo $BC = 0$, habentur valores dissipationum successivarum, pro quovis lentium numero:

$$1. \overset{I}{B}\overset{I}{C} = B^2 \times \frac{2n}{\overset{I}{N} - \overset{I}{I} \cdot \overset{I}{P}}.$$

$$2. \overset{II}{B}\overset{II}{C} = \overset{I}{B}^2 \times \frac{2n}{\overset{II}{N} - \overset{II}{I} \cdot \overset{II}{P}} + \frac{\overset{I}{B}^2 \cdot B^2}{\overset{I}{A}^2} \times \frac{2n}{\overset{II}{N} - \overset{II}{I} \cdot \overset{II}{P}}.$$

$$3. \overset{III}{B}\overset{III}{C} = \overset{II}{B}^2 \times \frac{2n}{\overset{III}{N} - \overset{III}{I} \cdot \overset{III}{P}} + \frac{\overset{II}{B}^2 \cdot \overset{I}{B}^2}{\overset{II}{A}^2} \times \frac{2n}{\overset{III}{N} - \overset{III}{I} \cdot \overset{III}{P}} + \frac{\overset{II}{B}^2 \cdot \overset{I}{B}^2 \cdot B^2}{\overset{II}{A}^2 \cdot \overset{I}{A}^2} \times \frac{2n}{\overset{III}{N} - \overset{III}{I} \cdot \overset{III}{P}}.$$

$$4. \overset{IV}{B}\overset{IV}{C} = \overset{III}{B}^2 \times \frac{2n}{\overset{IV}{N} - \overset{IV}{I} \cdot \overset{IV}{P}} + \frac{\overset{III}{B}^2 \cdot \overset{II}{B}^2}{\overset{III}{A}^2} \times \frac{2n}{\overset{IV}{N} - \overset{IV}{I} \cdot \overset{IV}{P}} + \frac{\overset{III}{B}^2 \cdot \overset{II}{B}^2 \cdot \overset{I}{B}^2}{\overset{III}{A}^2 \cdot \overset{II}{A}^2} \times \frac{2n}{\overset{IV}{N} - \overset{IV}{I} \cdot \overset{IV}{P}} + \frac{\overset{III}{B}^2 \cdot \overset{II}{B}^2 \cdot \overset{I}{B}^2 \cdot B^2}{\overset{III}{A}^2 \cdot \overset{II}{A}^2 \cdot \overset{I}{A}^2 \cdot A^2} \times \frac{2n}{\overset{IV}{N} - \overset{IV}{I} \cdot \overset{IV}{P}}.$$

Et ita deinceps, si plures fuerint lentes.

§ 18.

Datis positionibus lentium quocunque in communi axe, una cum singularum legibus refractionum pro radiis omnis generis, oporteat invenire relationem inter distantias focorum principalium earundem lentium, quæ efficiat, ut radii heterogenei à quovis puncto advenientes vel paralleli, post refractionem in omnibus lentibus emergant, sine dissipatione à diversa refrangibilitate radiorum oriunda, si fieri potest.

Solutio problematis generaliter absolvitur ponendo expressionem dissipationis, datæ multitudini lentium convenientem, § 17, nihilo æqualem, quo ipso habetur æquatio definiens relationum distantiarum focalium, P, P^I, P^{II}, P^{III} , &c. requisitam ad id, ut dissipatio in foco ultimo evanescat. Ita in casu binarum

$$\text{lentium habebitur: } \frac{\frac{I}{n}}{N - I \cdot P} + \frac{B^2}{A^2} \times \frac{n}{N - I \cdot P} = 0;$$

$$\text{in casu trium lentium erit: } \frac{\frac{II}{n}}{N - I \cdot P^{II}} + \frac{B^2}{A^2} \times \frac{\frac{I}{n}}{N - I \cdot P}$$

$$+ \frac{B^2 \cdot B^2}{A^2 \cdot A^2} \times \frac{n}{N - I \cdot P} = 0; \text{ in casu quatuor lentium:}$$

$$\frac{\frac{III}{n}}{N - I \times P^{III}} + \frac{B^2}{A^2} \times \frac{\frac{II}{n}}{N - I \times P^{II}} + \frac{B^2 \cdot B^2}{A^2 \cdot A^2} \times \frac{\frac{I}{n}}{N - I \times P}$$

$$+ \frac{B^2 \cdot B^2 \cdot B^2}{A^2 \cdot A^2 \cdot A^2} \times \frac{n}{N - I \times P} = 0; \text{ et ita porro, si plures}$$

fuerint lentes.

Ita

Ita si in æquatione pro binis lentibus, $\frac{n}{N - I \cdot P}$
 $+ \frac{B^2}{A^2} \times \frac{n}{N - I \cdot P} = 0$, ponatur distantia lentium
 $B + A = 0$, ut evadant contiguæ, quemadmodum
 in vitro objectivo Dollondiano; habetur $\frac{n}{N - I \cdot P}$
 $+ \frac{n}{N - I \cdot P} = 0$. Unde patet alterutram lentium P
 et P faciendam esse concavam, alteram convexam,
 et distantias earum focales affirmative sumtas P et P
 faciendas esse in ratione $\frac{n}{N - I}$ ad $\frac{n}{N - I}$, quæ ratio
 evadit n ad n , si vires refractivæ mediæ utriusque
 lentis fuerint æquales.

Similiter fit construendum telescopium ex tribus
 lentibus, quarum duæ priores P et P juxta se positæ
 constituent vitrum objectivum, tertia P fit ocularis,
 et potentia amplificandi exprimitur numero m .
 Quoniam lentes priores P et P sunt juxta se positæ,
 erit earum distantia $B + A = 0$. Quoniam per con-
 stitutionem telescopii radii in primam lentem inci-
 dentes sunt paralleli, erit $B = P$, et $\frac{I}{B} = \frac{I}{P} + \frac{I}{P}$.
 Quoniam radii ex ultima lente emergunt paralleli,
 erit $A = P$. Et tandem, quia potentia amplificandi
 5 indicatur

indicatur numero m , erit $\overset{I}{B} = m \overset{II}{P}$. Substitutis itaque his valoribus in æquatione pro tribus lentibus

supra allata, tranfit illa in hanc: $\frac{n}{\overset{II}{N} - \overset{I}{I}} \cdot \overset{I}{P} + \frac{\overset{I}{n}}{\overset{I}{N} - \overset{I}{I}} \cdot \overset{I}{P}$

$+ \frac{\overset{II}{n}}{\overset{II}{N} - \overset{II}{I}} \cdot m^2 \overset{II}{P} = 0$. Et hæc æquatio una cum æqua-

tione $\frac{\overset{I}{I}}{\overset{I}{P}} + \frac{\overset{I}{I}}{\overset{I}{P}} - \frac{\overset{I}{I}}{m \overset{II}{P}} = 0$, determinat rectationem

distantiarum focalium $\overset{I}{P}$, $\overset{II}{P}$, $\overset{II}{P}$, qualis ad id requiritur, ut radiorum emergentium dissipatio à diversa refrangibilitate nulla fit. Comparando scilicet has

æquationes, habetur $\left(\frac{n m}{\overset{II}{N} - \overset{II}{I}} + \frac{\overset{II}{n}}{\overset{II}{N} - \overset{II}{I}} \right) \overset{I}{P} + \left(\frac{\overset{I}{n} m}{\overset{I}{N} - \overset{I}{I}} \right.$

$\left. \frac{\overset{II}{n}}{\overset{II}{N} - \overset{II}{I}} \right) \overset{II}{P} = 0$, et $\left(\frac{n}{\overset{II}{N} - \overset{II}{I}} - \frac{\overset{I}{n}}{\overset{I}{N} - \overset{I}{I}} \right) \times m^2 \overset{II}{P}$

$+ \left(\frac{\overset{I}{n} m}{\overset{I}{N} - \overset{I}{I}} + \frac{\overset{II}{n}}{\overset{II}{N} - \overset{II}{I}} \right) \times \overset{I}{P} = 0$. Eadem ratione pro-

cedendum est in aliis casibus.

S. Klingenskierna.

Stockholm,
20 Aprilis 1760.

ERRATUM.

Page 900, Line 28, *after* as usual *add* and the
poker *plus*.

A N

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